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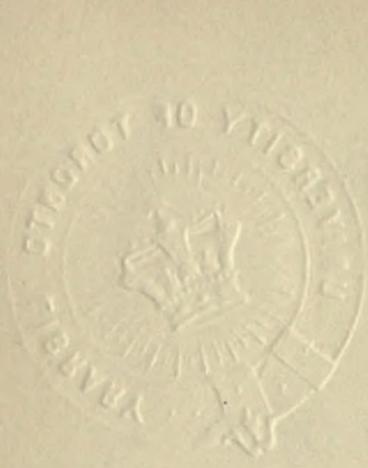PUBLISHED BY HARVARD UNIVERSITY
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PREFATORY NOTE

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A PRELIMINARY STUDY OF CERTAIN MANUSCRIPTS OF SUETONIUS' LIVES OF THE CAESARS

By Clement Lawrence Smith

SECOND PAPER

IN my first paper under this title[1] I published the results of an examination of some thirty manuscripts of the *De Vita Caesarum* and traced, as far as the available evidence warranted, the relations of these to the older and better known manuscripts and to one another. Without attempting a complete classification, for which my material was manifestly inadequate, I showed that the majority of the manuscripts examined by me belong to a different class from that of Codex Memmianus (A); and in each of the two classes thus distinguished[2] — to which I assigned seven and twenty-one manuscripts respectively — I was able to recognize two subordinate groups: in the first class a group represented by Memmianus itself, and one represented by the Third Medicean (M^3);[3] in the second class a 'Florentine' group of seven

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[1] *Harvard Studies*, vol. XII (1901), pp. 19–58. In referring to that paper I shall cite simply the volume and page of the *Studies*.

[2] Quite independently, and on a much broader basis of research, L. Preud'homme, in his important *Troisième étude sur l'histoire du texte de Suétone de vita Caesarum* (Brussels, 1904), has reached a similar result, and finds the same division running through about 125 manuscripts. His two classes are essentially identical with mine, and we differ only in the assignment of a few individual manuscripts: V^6 and V^{35}, which I placed in the second class, and V^5, V^{13}, V^{14}, and V^{38}, which I left on the border between the two classes, though recognizing their closer affinity to the first, are by him definitely assigned to the first class. Of the manuscripts which, for lack of sufficient evidence, I refrained from classifying, M. Preud'homme assigns P^1, Ven^2, and Z to Class I; Ven^1 and Ven^3 to Class II. One manuscript of my first class and four of my second (R^1; $V^1 R^2 B^3 B^4$) I do not find anywhere in his list. Of the manuscripts discussed in the present paper our classification agrees in the case of all except B^7, which M. Preud'homme is partly in error, as I shall show (page 11), in assigning to the second class.

[3] M. Preud'homme also recognizes in M^3 and its congeners a distinct group, in which he includes (besides M^1) Parisinus 5801 and Montepessulanus 117 (*Troisième étude*, page 24).

copies dating from the XII.–XIV. centuries, and an 'Urbinas' group of seven XV. century manuscripts. A brief sojourn in Europe in the spring of 1903 gave me the opportunity to make some further observations in this field, the results of which I will here set forth.

In the Library of the Vatican

Vaticanus Latinus 6396. Parchment, 4°, XV. century.

A very brief examination of this manuscript, which escaped my notice in 1898, not being found in the Catalogue,[1] sufficed to show that it belongs to the Urbinas group. Its agreement with *U* itself is so constant that in the 46 excerpts from the *Julius* in which both are represented there is only one case of divergence, and that of no significance: 31, 31 quodam (for quondam).

V[4] Vaticanus Latinus 1904.

My attention having been called to some errors in the collation of this manuscript which I used in preparing my first paper,[2] I took the opportunity to verify all my excerpts and found them inaccurate in twelve places, where the correct reading is as follows: 32, 26 libris *om.*; 39, 18 confiteri; 42, 16 pansa equidem ad eos; 44, 31 cleopatra liberis; 46, 5 integri temasini; 52, 36 ad exemplar; 53, 34 senatores; 55, 36 in exprobratis; 58, 11 magnorum; 74, 28 tiburi; 80, 34 demisso e caelo; 85, 30 altero quae.

These corrections disarrange my examples somewhat, — for instance, the list of twelve places in which I held that *V*[4] (against *A*) was 'certainly right'[3] must be reduced to eight; but they do not materially affect the evidence by which I showed the close relation of *V*[4] to *A* on the one hand, and to *M*[8] on the other;[4] nor do they disturb my conclusion that *V*[4], though standing nearer to the Medicean than to the Memmianus group,[5] is not to be included in either. Its relation to

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[1] See vol. XII, p. 21, footnote.

[2] I am indebted to Professor Ihm for calling my attention to the inaccuracy of this collation.

[3] Vol. XII, p. 46.

[4] Vol. XII, pp. 44–48.

[5] This is also the view of M. Preud'homme, in whose scheme (*Trois. étude,* pp. 24 ff., 61) *V*[4] and the archetype (x′) of what I have called the Medicean group have their common source in a manuscript (x) closely related to *A*.

the two groups is neatly illustrated by the variants at 91, 29, where the archetype apparently had EXSERVISSEIVS.. In the Memmianus group this has been transformed by haplography into *ex seruis eius*. M^8 has preserved, with a slight change of spelling, the right reading, *exeruisse ius*, corrected in R^1, a later manuscript of the Medicean group, to the more intelligible *exercuisse ius*. V^4 agrees with M^8 in preserving the verb, but goes its own way and gives us, by dittography, *exseruisse eius*. The independence of V^4 is vouched for by such examples as the following :

10, 6 licerentur $A V^4$ ducerentur M^8
11, 23 alias addit $A V^4$ aliis additis M^8
19, 22 exulabant $A V^4$ exularent M^8
22, 29 uasa $A V^4$ ut uasa M^8
25, 18 et ait uero $A V^4$ uero M^8
27, 29 conibebat $A V^4$ cohibebat M^8
32, 26 detineretur $A V^4$ contineretur M^8
35, 1 maximo aequintus $A V^4$ maximae quintus M^8
38, 14 minos gentes $A V^4$ minis gentis M^8
49, 15 ut c $\cdot A V^4$ et brutum M^8
50, 28 praesident idem $A V^4$ prae se identidem M^8
55, 23 uirili toga $A V^4$ uirilem togam M^8
62, 10 sedulo lentius $A V^4$ sedulo uiolentius M^8
73, 32 etiam memoriam $A V^4$ etiam in memoriam M^8
80, 21 in eius signum rei p $\cdot$ quam $A V^4$ in sinum eius signum rei p $\cdot$ quod M^8
85, 16 quinques $A V^4$ quinque M^8

7, 6 adiutore A auctore $V^4 M^8$
11, 17 consulatus A consularis $V^4 M^8$
18, 19 ac A et $V^4 M^8$
26, 32 moranti se A moranti $V^4 M^8$
33, 35 laureo A aureo $V^4 M^8$
36, 18 adinspectantium A inspectantium $V^4 M^8$
38, 16 magno inter A magno interuallo $V^4 M^8$
39, 18 profiteri A confiteri $V^4 M^8$
46, 29 abim A abin $V^4 M^8$
50, 21 primum A primo $V^4 M^8$

61, 35 ne *A*   et ne *V*⁴ *M*⁸
67, 37 impatientiam *A*   patientiam *V*⁴ *M*⁸
71, 4 in urbem *A*   extra urbem *V*⁴ *M*⁸
74, 37 primam *A*   pilam *V*⁴ *M*⁸
83, 11 rerumque missilia *A*   rerumque missilium *V*⁴ *M*⁸

In the Ambrosian Library at Milan

Am Ambrosianus H. 90.   Parchment, small folio, early XV. century.
     Ambrosianus H. 144.   Paper, 4°, early XV. century (1432).

The first of these two I excerpted fully through the *Julius*. It is a
manuscript of the second class, akin to *B*⁸ and *B*⁴, with which it will be
discussed below. The second is in a deplorable state, largely moth-
eaten, and apparently still food for moths. I made no excerpts.

In the British Museum

*B*⁴ Lat. Cl. 12009.   Parchment, XV. century.

This manuscript, which for lack of time I had been obliged to pass
over in 1898, I excerpted for the *Julius*. It proves to be of the second
class, and closely related to *B*⁸ (Lat. Cl. 31914); and in these two,
together with the Ambrosian codex (*Am*) mentioned above, and prob-
ably the Leyden codex (*L*), we appear to have come upon a third
group in the second class. The Milan and London codices are found
to agree, in the excerpts in which comparison can be made, about five
times out of six (*Am B*⁸ 51 : 10; *Am B*⁴ 54 : 10; *B*⁸ *B*⁴ 48 : 12). In
the case of *L* the amount of divergence is greater (*Am L* 39 : 10;
*B*⁸ *L* 63 : 18; *B*⁴ *L* 37 : 14), owing to the fact that *L* has acquired a
number of readings peculiar to itself, such as 11, 23 alias publico; 24,
5 eloquentis; 50, 28 praesedens identidem prae se; 55, 36 ex impro-
batis; 56, 10 romani populi. These figures are by no means decisive,
but the conclusion to which they point is confirmed on a closer inspec-
tion of the various readings.[1] The cases in my excerpts from the *Julius*
in which *Am B*⁸ *B*⁴ fail to agree are as follows:

   3, 8 iulius caesar annum agens *Am B*⁴   annum iulius caesar agens
*B*⁸   annum agens caesar *L*
   4, 5 regiae *B*⁸   reginae *B*⁴   regi *Am L*

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[1] For the excerpts from *B*⁸ and *L* see vol. XII, p. 24 ff.

9, 37 quicquam *Am*   quidquam *L*   quiddam B^3 B^4
10, 32 uaticinia *Am* B^3 uatinia B^4
11, 1 assiria *Am* B^4 assyria B^3 *L*
12, 18 et *Am L*   sed B^3 B^4
13, 2 prosequebatur *Am* B^4 *L*   persequebatur B^3
14, 16 aduentu fecerat suo *Am* B^4 aduentu suo fecerat B^3 *L*
14, 37 imperii *Am* B^4 *L*   *om.* B^3 regni B_2^3
22, 29 ut uasa *Am* B^3 ut nasa B^4 et uasa *L*
23, 19 a proconsule *Am* B^3 *L*   proconsule B^4
27, 29 conniuebat *Am* B^3 *L*   conuiuebat B^4
31, 26 amplius *Am*   amprius B^3 B^4 *L*
31, 31 exta sacra *Am* B^3 *L*   exacta sacra B^4
32, 28 sententia causa *Am L*   sententia tam B^4 summa causa B^3

Some of these are of the nature of exceptions that prove the rule, as 4, 5 and 27, 29, where a reading which B^3 shares with a very small number of manuscripts appears slightly varied in B^4. Three or four at most might be held to prove the influence of other manuscripts. Nearly all readily explain themselves as the result of easy errors or intentional corrections or similar causes which are at work in the making of every manuscript. They have little force against the positive evidence of the list that follows, which includes those places only in which *Am* B^3 B^4, and usually *L*, are found in agreement against one or both of the other groups of the second class:

4, 17 et triumphalem uirum *Am* B^3 B^4 *L Flor*   triumphalemque uirum *Urb*
4, 34 tribunatu *Am* B^3 B^4 *L*   tribunatum *Flor Urb*
4, 35 auctores *Am* B^3 B^4 *L*   auctoresque *Flor Urb*
5, 18 mandatu *Flor*   mandato *Am* B^3 B^4 *L Urb*
9, 23 accensus *Am* B^3 B^4 *L Flor*   accensus funis (or ter funis) *Urb*
11, 17 consulatu *Flor Urb*   consularis *Am* B^3 B^4
12, 18 et *Am* B^3 B^4 *L Urb*   sed *Flor*
12, 33 praedia *Am* B^3 B^4 *Flor*   prandia *Urb*
13, 34 temptante *Am* B^3 B^4 *Flor*   tempestate *Urb L*
15, 27 exhortandoque *Am* B^3 B^4 *Flor*   adhortandoque (or -dum-que) *Urb*
24, 25 caesaris *Am* B^3 B^4 *L Urb*   *om. Flor*

26, 32 moranti cuspide *Am B*⁸ *B*⁴ *L*   moranti secus cuspide *Flor Urb*

29, 18 et in *Am B*⁸ *B*⁴ *L*   in *Flor Urb*

30, 34 notas *Am B*⁸ *B*⁴ *L Urb*   nota *Flor*

32, 14 seu parum *Flor Urb*   seu propter parum *Am B*⁸ *B*⁴ *L*

33, 25 iulo *Am B*⁸ *B*⁴ *L*   iulio *Urb*   ilio *Flor*

33, 27 cuius rei *Urb*   quam rem *Am B*⁸ *B*⁴ *Flor*

We have here sufficient evidence to show that *Am B*⁸ *B*⁴ *L*, which for convenience I shall call the Ambrosian group, represent a tradition to some extent distinct from those represented by the other groups of the second class. To illustrate now how the several groups of both classes array themselves in support of various readings I give the following examples from the *Julius :*

4, 17 et triumphalem *Class I*   et triumphalem uirum *Flor Ambr* triumphalemque uirum *Urb*

4, 30 desidere *Mem Flor Urb Ambr*   dissidere *V*⁴ *Med*

4, 34 tribunatu *Class I Ambr*   tribunatum *Flor Urb*

4, 35 actores *Class I*   auctores *Ambr*   auctoresque *Flor Urb*

9, 8 pollicendi *Flor Urb Ambr*   pollicenti *Class I*

9, 23 accensus *Class I Flor Ambr*   accensus funis (or ter funis) *Urb*

10, 6 licerentur *Mem V*⁴ *Flor Urb Ambr*   ducerentur *Med*

11, 17 consulatus *Mem*   consulatu *Flor Urb*   consularis *V*⁴ *Med Ambr*

11, 23 alias priuato sumptu addidit *Flor Urb Ambr*   a. p. s. addit *Mem V*⁴   aliis p. s. additis *Med*

12, 18 et *Urb Ambr*   sed *Class I Flor*

12, 33 praedia *Class I Flor Ambr*   prandia *Urb*

16, 12 et fugientem *Flor Urb Ambr*   effugientem *Class I*

17, 9 dextra atque *Flor Urb Ambr*   dextraque *Class I*

18, 4 regione *Class I*   in regione *Flor Urb Ambr*

18, 19 ac *Mem Flor Urb Ambr*   et *V*⁴ *Med*

22, 6 ecce — gallias *Flor Urb Ambr*   om. *Class I*

22, 27 aurum . . . effutuisti *Class I*   auro . . . stuprum emisti *Flor Urb Ambr*

24, 2 oratorum *Flor Urb Ambr*   oratorem *Class I*

24, 25 caesaris *Class I Urb Ambr*   *om. Flor*

29, 18 deuersorio (or diuersorio) loco *Class I*   in div. loco *Flor Urb*   et in div. loco *Ambr*

30, 34 notas *Mem Med Urb Ambr*   nota *V⁴ Flor*

32, 14 seu parum *Class I Flor Urb*   seu propter parum *Ambr*

32, 26 libris *Flor Urb Ambr*   *om. Class I*

33, 25 iulo *Med Ambr*   iulio *V⁴ Urb*   ilio *Mem Flor*

33, 27 cuius rei *Class I Urb*   quam rem *Flor Ambr*

Of the remaining manuscripts in the British Museum[1] I selected for examination in the limited time at my disposal these two :

*B*⁶ Lat. Class. Arundel 32.   Paper, 4°, XV. century.
*B*⁷ Lat. Class. 21098.   Paper, small 4°, XV. century.

The Arundel manuscripts in the Museum were transferred to that institution from the Royal Society in 1831.   The largest part of them, according to the Catalogue, were purchased by the Earl of Arundel in Nuremberg in 1636.   That *B*⁶ was in this Nuremberg collection is not stated, but is not improbable.   It is of Italian origin, the writer giving his name as ' M. Terentius lunensis.'   It contains the twelve lives, as twelve books, occupying 113 leaves, with 33 lines to a page.   It is written in a plain, business-like, semi-cursive hand, with no attempt at caligraphy or ornamentation of any kind, except here and there a dash of red drawn obliquely across a letter — usually a capital[2] — and superscriptions in red.   Space has been left at the beginning of each life for a large capital initial, but has not been filled.   Otherwise the text is written solid, interrupted by no subject-headings or paragraph marks. There are marginal capitula, some in red, some in black by a later hand.   There are no corrections except a few in the text by the writer himself.   The Greek passages are omitted entirely, sometimes ignored ;

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[1] The Museum possesses thirteen manuscripts of the *Lives:* Regius 15.C.III, XII. century; Regius 15.C.IV.1, early XIII. century; and the following of the XV. century: Lat. Class. 12009, 12010, 21098, 24913, 31914, Arundel 32, Lansdowne 838, Harley 5342.64a, Burney 259 and 260, Sloane 2509.

[2] This trick of the scribes has the effect of making the letter *i* resemble *y*, and is no doubt responsible in many cases for such misspellings as *tyberius, ymagine, Ynfanti, Yre,* etc.

usually, however, a blank space has been left, often with a Latin version written over it in red or after it in black.

The insertion of a Latin version of the Greek passages and the division into twelve books are features which the Arundel codex has in common with the Third Medicean, and these indications of kinship with that codex are confirmed by the evidence of the excerpts, which assigns to B^6 a place somewhat similar to that of R^1 in the Medicean group of the first class. While exhibiting unmistakable traces of its derivation from the archetype of Class I, it has suffered numerous 'corrections' from other sources. The following are some of the characteristic readings of Class I which it retains: 4, 35 actores; 18, 4 regione; 34, 8 ouio; 39, 29 carmensis; 47, 3 ultus est; 48, 3 ignominia; 48, 18 urbis; 49, 17 decimum; 52, 25 flamonium; 53, 35 tabulas; 57, 29 partimque; 89, 18 troiam; 114, 30 patria; 163, 28 defuit et ueterum; 183, 27 ueste; 227, 12 antiqua erat; 238, 11 aduerso rumore; 240, 20 uerum. Where the reading of the archetype was manifestly wrong or meaningless it has been corrected; thus we find 14, 16 aduentu suo (for sui); 21, 30 ad uinum (for ui); etc. In a few cases these 'corrections' may have been independent, but they are usually derived from some manuscript of the second class, as 4, 17 triumphalem uirum (for triumphalem); 21, 6 recentiora (for retiora); 22, 6 ecce — gallias, etc.

In spite of these corrections and corruptions B^6 maintains a distinct preponderance of agreement with A (47 : 38), and still more so with M^3 (54 : 28). Where A and M^3 part company B^6 sides with A against M^3 in a few cases:

4, 30 desidere $A\ B^6$	dissidere M^3	
10, 32 uatinia $A\ B^6$	uaticinia M^3	
19, 22 exulabat B^6	exulabant A	exularent M^3
34, 8 ouio $A\ B^6$	obuio M^3	
50, 21 primum $A\ B^6$	primo M^3	
71, 4 in $A\ B^6$	extra M^3	
85, 30 quem $A\ B^6$	quae M^3	
131, 33 notos in bonam partem $A\ B^6$		notos M^3

Much more numerous and more significant are the places in which B^6 agrees with M^3 against A. I have counted in my excerpts fifty-four such places, of which the following may serve as examples:

7, 6 auctore $M^3 B^6$ adiutore A
10, 6 ducerentur $M^3 B^6$ licerentur A
11, 23 aliis . . . additis $M^3 B^6$ alias . . . addit A
26, 32 moranti cuspide $M^3 B^6$ moranti se cuspide A
33, 25 iulo M^3 iulio B^6 ilio A
33, 35 aureo $M^3 B^6$ laureo A
38, 14 minis gentis $M^3 B^6$ minos gentes A
39, 18 confiteri $M^3 B^6$ profiteri A
40, 30 autem $M^3 B^6$ eum A
42, 3 hac fraude $M^3 B^6$ ac fraude A
44, 31 de cleopatrae liberis $M^3 B^6$ de cleopatra liberis A
50, 28 prae se identidem $M^3 B^6$ praesident idem A
55, 23 uirilem togam $M^3 B^6$ uirili toga A
78, 9 ipse $M^3 B^6$ ipse ēi A
80, 21 in sinum eius signum rei p · quod $M^3 B^6$ in eius signum rei p · quam A
83, 11 rerumque missilium $M^3 B^6$ rerumque missilia A
85, 16 quinque $M^3 B^6$ quinques A
93, 11 ignota $M^3 B^6$ ignoti A
141, 1 omnem urbem $M^3 B^6$ urbem omnem A
147, 13 magna ui $M^3 B^6$ naui A
192, 4 male fracto $M^3 B^6$ male facto A
211, 16 erat $M^3 B^6$ tulerat A
217, 27 suae $M^3 B^6$ uenetae A
224, 7 incertum sponte an $M^3 B^6$ an A
253, 9 canum $M^3 B^6$ incanum A

In orthography B^6 is distinctly superior to the average mediaeval or renaissance manuscript. Not that there is any lack of the ordinary mistakes of copying; but its real errors of orthography are for the most part confined to those that arise from the confusion of *c* and *t* or *i* and *y*, or from the misplacement of the aspirate. Thus we find both *acies* and *aties*, *otium* and *ocium; contio, concio,* and *conctio; sectius, suspic- tiones; dimictebat, actritus; prouintia* (found in A also); *circha, cathenati, cohercuerat, introhire; ymagine, tyberius, silla;*[1] and there are the usual distortions of proper names. But there are no barbarisms

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[1] See above, p. 7, footnote.

like *michi* and *nichil;* and, on the other hand, we meet here and there
with forms which we must recognize as survivals from the archetype.[1]
I have noted a few examples. The accusative plural of *i*-stems usually
ends in *-es*, but often in *-is*, as 31, 19 pluris; 38, 14 gentis (in which
the ending has been protected by the corrupt form of the adjective,
minis); 39, 35 octobris; 205, 5 recusantis; 205, 6 flagitantis. In
nouns and adjectives with stems in *-io-*, *-ia-*, a single *-i-*, for *-ii-*, is
found in a few places, as 12, 1 stipendi; 211, 31 lecticaris; likewise in
verb forms from perfect stems in *-iui-*, as 4, 13 redit; 14, 28 petissem;
42, 12 subisse. Compounds of *iacio* usually have *-ii-*, but *-i-* is not
infrequent, as 26, 36 traiceret; 33, 13 deicerent; 33, 20 disicerent;
40, 11 obiciatur; 42, 18 adicit, etc. Prepositional prefixes are ordi-
narily assimilated, but the unmodified form often occurs, as 15, 28
adfirmaret; 18, 22 adlegit; 26, 12 obprobrium; 207, 9 adsimilis; 211,
16 adfuturi; 214, 22 adnexo, etc. There are a few instances of *-qu-*
for *-cu-*, as 4, 38 sequti; 207, 22 alloquturi. Most striking of all is the
spelling of the temporal conjunction *cum*, which is regularly written
quom, while the preposition is regularly written *cum*. The unfamiliar
distinction has not been observed with entire consistency by the scribes,
who have sometimes lapsed into *cum* for the conjunction and sometimes
overdone the matter by writing the preposition *quom*. Thus in the
Caligula and *Claudius* together, out of 79 occurrences of the conjunc-
tion (leaving out of the count a few cases where the text is corrupt), it
is written *quom* 62 times, and *cum* 17 times; while the preposition,
which occurs 43 times, appears as *cum* in all cases except four: 139, 21
quom exigua manu; 139, 37 quom equitatu; 148, 33 simul quom
fratre; 153, 23 quom cetera turba. In this respect our manuscript
bears some resemblance to the palimpsest of Fronto.

The other forms of which examples are given in the foregoing para-
graph are undoubtedly survivals of sound tradition. Is *quom* likewise

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[1] See Roth, Praef. xxxvi. Roth is no doubt right in accepting the more archaic
forms found in *A* as authentic. Whether Suetonius used the archaic and the contem-
porary conventional forms of the same word indifferently is another question. The
mixed spelling of *A* may well be due to the gradual encroachment of the conventional
orthography of the scribes on the peculiar spelling of Suetonius (cf. Preud'homme,
Troisième étude, pp. 20 f.), and *B*[6] may represent a more advanced stage of the
same process.

traditional, or does it merely represent the unwarranted opinion of some mediaeval scholar? The testimony of our manuscript rather suggests than answers this question. Of this much, I think, we may rest assured: the form was not first introduced into B^6. It is plain that the writer of that manuscript had no well defined views on orthography; he copied what he found in his original. Back of this we cannot go. There is nothing intrinsically improbable in the supposition that Suetonius, like some of his contemporaries,[1] distinguished the conjunction from the preposition by writing it *quom*, and that this spelling has disappeared in nearly all of our manuscripts; but the unsupported testimony of B^6 cannot be said to carry us very far on the way to such a conclusion. The whole question of Suetonian orthography, however, deserves a more thorough investigation than it has yet received, and to that end I have set down the evidence of B^6 for whatever it may be worth.

The other manuscript which I examined, B^7, is a composite production both in its handwriting and in its text. It is written in six unequal instalments, apparently by as many different scribes; and the text is derived from two very different sources. As far as 97, 33, where the work of the second scribe ends, and perhaps a little farther, the text belongs to the Medicean group of the first class; while from this point to 230, 7 grauissima increpuit (*Vesp.* 8), where it breaks off incomplete, it belongs to the Florentine group of the second class. As the text stops at the end of a quire, we may suppose the copy was once complete and has suffered the loss of one quire. There has also been some disarrangement of the leaves: the order of foll. 28–34 should be 29, 28, 31, 30, 32, 34, 33. In the part of the text belonging to the first class the Greek passages are given, sometimes with a Latin version superscribed, as in V^4 and M^8; in the other part the spaces left for the Greek remain unfilled.

The first part, like B^6 and R^1, is a much corrected text, which still retains a decided preponderance of agreement with the representative manuscripts of the first class, particularly with M^8 ($B^7 A$ 44 : 24; $B^7 V^4$ 48 : 20; $B^7 M^8$ 56 : 12), as well as more specific evidence of its origin in numerous survivals of characteristic readings of that class.

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[1] Cf. Terentius Scaurus, VII, p. 28 K.

The following may serve as examples: 4, 35 actores; 22, 27 aurum . . . effutuisti; 31, 31 exta quondam; 32, 26 libris *om.*; 47, 3 ultus est; 48, 18 urbis; 49, 17 decimum; 52, 4 ut qui; 52, 25 flamonium; 53, 3 exempli; 58, 11 puerorum; 74, 28 tiburi; 85, 11 sextam; 85, 28 tribus; 86, 10 atque; 89, 18 troiam.

Where *A* and *M*⁸ disagree, *B*⁷ is found in pretty constant agreement with the latter. There are 42 such cases in my excerpts from this part of the text, in two of which *B*⁷ differs from both of the older manuscripts. It sides with *A* in four places: 4, 30 desidere; 10, 6 licerentur; 10, 32 uatinia; 19, 22 exulabant. In the remaining 36 places it sides with *M*⁸ against *A*. I forbear to give examples, which would be mainly a repetition of the list given above[1] for *B*⁶. In 4, 5 regiae, *B*⁷ approaches *V*⁴, but I have noticed no other indication of special resemblance to that manuscript. It clearly belongs, in this part of its text, to the Medicean group.

In the other part (from 97, 33, or thereabouts, to the end) *B*⁷ is closely related to Vaticanus 1860 (*V*⁰). In 64 excerpts in which both manuscripts are represented there are only seven cases of disagreement, and these are evidently due in every instance to unconscious error, with the possible exception of 194, 19 praefectorum (for peccatorum). On the other hand, *B*⁷ faithfully follows *V*⁰ in all its peculiar readings, good and bad, such as 111, 27 cum coniugibus; 127, 18 quercina; 188, 38 perusurum; 192, 4 labefacto; 208, 19 addidit; 214, 19 claudusque; 217, 27 ueterem, etc.; it reproduces the grosser interpolations of *V*⁰, of which I gave some examples in my account of that manuscript;[2] and finally it has the same transposition in the *Galba*, with the passages in the same order.[3] From all this we may infer that *B*⁷, in this part of its text, was copied directly either from *V*⁰ or, more probably, from the common archetype of *V*⁰ and *V*¹.

A word, in conclusion, about the value of the XV. century manuscripts, which have engaged a large share of our attention in these studies, and to which — for that reason, I suppose — one or two of my

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[1] Page 9.

[2] Vol. XII, p. 50.

[3] See vol. XII, p. 48. The part of the *Vespasian* in which the great lacuna of *V*⁰ ¹ occurs was in the lost quire of *B*⁷.

excellent critics think I attach undue importance. In point of fact, I have nowhere attempted to say how much importance should be assigned to them, nor am I yet prepared to do so. I certainly cannot acquiesce in the comfortable assurance of Roth, that these manuscripts may be safely neglected, and I adduced some proof of the unsoundness of this view. But no proof ought to be needed. Roth, quite arbitrarily, set apart the XV. century manuscripts as a class by themselves, and from a somewhat imperfect acquaintance with a few of them pronounced them one and all of no authority, — surely a most unscientific procedure. Manuscripts cannot be assembled in lots, like so much merchandise, and priced by samples. They must be examined, each for itself, and their relations so far as possible determined; and when this has been done it will be time to pronounce judgment on their value and importance.

In the present instance such an examination is, in my opinion, distinctly worth while, and may prove fruitful in two ways. It may bring to light individual manuscripts of exceptional value, as being directly copied from originals of much earlier date. We have an example of this kind in Monacensis, a paper codex of most unpromising appearance, which proved on examination to be a faithful reproduction of a text written at least four centuries before.[1] It happens in this instance that we have a much older representative of the same text in Gudianus 268 (G^2), of the XI. century, so that the discovery of the character of Monacensis did not add materially to our resources; but it may at least serve to suggest that in the multitude of XV. century manuscripts there may be others of the same sort, which are the *sole* representatives of their much earlier originals. Again, if we can establish groups and sub-classes among these manuscripts, of which I have pointed out clear indications, we can work back to an earlier stage of the tradition in that way. And we cannot well dispense with any aid these late manuscripts may have to give us. In the first class our need is not so very urgent; for here we are fairly well provided with older

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[1] Whether it is a copy of G^2, as Professor Ihm thinks (Hermes 37, 593 ff.), or of the original of G^2, as I was 'disposed to think, while waiting for further evidence' (vol. XII, p. 42). Professor Ihm is at much pains to refute what he calls my 'hypothesis,' and contributes some additional evidence, but his arguments are hardly conclusive.

copies, headed by Memmianus, whose preëminence no one disputes, though it must be used with discrimination, as indeed Roth used it; but in the second class, where we have nothing earlier than the XII. century, it may easily happen that a single XV. century manuscript, or a group of them, will prove to be quite as good a representative of the archetype of the class as any of the older copies. The second class, in fact, is still, for the most part, an undigested mass, which must be organized in some fashion before we can make effective use of it; and in working out this problem the XV. century manuscripts, which make up the great majority of the class, may by no means be left out of the account.

For these reasons the exploration of the manuscripts should go on; and as there will be no opportunity for me to take any further part in it, I may here express the hope that the work may be taken up by other hands and carried to completion.

THE DRAMATIC ART OF AESCHYLUS[1]

By Chandler R. Post

THE purpose of this paper is to consider the development of the dramatic art of Aeschylus in the narrow and technical sense of the term "dramatic art." Such a limitation excludes of necessity all reference to the poetic, philosophic, or religious aspects of his works, except in so far as reasons arising from these characteristics influenced the construction of his dramas or guided him in the molding of character. Thus, in the *Seven against Thebes*, I believe that Aeschylus's sense of justice and his confidence in a Power whose ideals surpass the conceptions of mortals led him to add the much discussed lines of Antigone, in fear that up to this point he had not made his meaning clear; and in this case a discussion of the moving principles of the tragedy would rightly find a place in a paper whose primal purpose is to consider the technical qualities of the dramas of Aeschylus. Or again, in the *Agamemnon*, purely from the point of view of the architecture of the play, the references to Menelaus, and the following chorus form an unessential and hence a false member, which is to be explained, I think, only by taking into account the aesthetic side of the author's character. Such questions can properly demand consideration in a paper whose scope is limited to what may be called the mere mechanical aspect of the playwright's work, though this mechanical labor is in the last analysis to be referred to the intellectual part of the man and is that thing which really distinguishes the dramatist from the lyric or epic poet.

Dramatic art, in the narrow sense, naturally divides itself into two heads: the construction of the framework of the drama, and the filling in of this framework by characterization. Here the building figure fails, for the dramatist has to create the material which an actual constructor would find ready at hand. Characterization is an essential part of the

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[1] This article is the thesis presented by the successful candidate for the Charles Eliot Norton Fellowship in Greek Studies.

technical construction, for the audience requires not only that the framework of events form a solid edifice, but also that the covering of this framework exhibit a consistent harmony of division with division, and as a whole betray no incongruity with the structure, the bareness and cold calculation of which it is meant to conceal. In addition to construction and delineation, there are often certain uncategorized qualities, which nevertheless are illustrations of the dramatist's method of playbuilding. Such a quality is the use which Aeschylus makes of ghosts. Under these three heads of construction, of character, of general topics, the discussion of the points in each play will find a place.

The method I wish to adopt is a separate consideration of each drama, with an effort to discover what position each holds in relation to the three headings which I have just mentioned, and how the plays are connected one with another in the development of the art of Aeschylus. Such a mode of procedure, will, I hope, show Aeschylus, in the earlier works, making experiments in the several departments of the dramatist's art, studying in one play one device, in another, another, until finally, conscious of a mastery over the technical details, he combines in the later works the results of all the lessons that he has learned and fashions his material according to his will. Aeschylus had much the same problem presented to him as to the Florentine painters of the first seventy-five years of the Quattrocento, each of whom seems to have devoted himself almost exclusively to the study of some separate aspect of his profession. Thus we find Paolo Uccello paying particular attention to the mathematical questions of perspective, Andrea del Castagno to the expression of power, Domenico Veneziano to the individualization of types, Fra Filippo Lippi to the expression of sentiment, Antonio Pollaiuolo and Andrea Verrochio to the nude, to landscape, and especially to movement. The later painters took advantage of what the earlier had studied and learned, and combining several of their characteristics brought Italian art to its culmination. In the case of Aeschylus, we cannot from the meagre fragments of his predecessors ascertain how much he gleaned from them, but in his own works we can see him studying one aspect and then another, until joining together all his acquired knowledge he reaches the summit of his power in such works as the *Prometheus* and the *Oresteia*.

But before the plays can be treated separately certain reservations must be made, if we are to start on a firm and intelligible basis.

In the first place, the Greek dramatist was confronted by a much more intricate problem in the exposition of the circumstances of the play than the playwright of the present day. It was the established rule in the time of Aeschylus that the action of the drama should exhibit only the result, the catastrophe of a certain chain of misfortunes, not the events themselves. Whether Phrynichus (whose plays seem to have been built upon a single fundamental incident, decorated flamboyantly by a very considerable amount of choral lyrics) had influence in fixing this tradition, or whether Aeschylus himself deliberately chose this characteristic for his dramas, makes little difference ; it is certain that his plays are all illustrations of this method and that his successors submitted themselves to this precedent.

Thus, if with Dindorf we judge the *Suppliants* to be the first of the trilogy (and I do not see how we can well do otherwise, when we take into account the very elaborate exposition which is found in the opening of the play), the betrothal of the daughters of Danaus has already taken place, and it is this act which precipitates the action of the *Suppliants* and the final catastrophe of the whole trilogy.

In the *Persians* not only have deeds occurred which lead up to the catastrophe, but even the catastrophe itself. Darius has insulted Hellenic freedom, and Xerxes, his son, has not only offended by a haughtier insolence against a mortal race, but has dared to measure his strength with the immortal gods, lashing and fettering the waves of Poseidon at the Hellespont, and disfiguring the face of Nature at Mt. Athos. Retribution for these crimes, that is, the catastrophe arising from this chain of sin, falls upon the Persians at Salamis by the hands of the Gods of the Sea, and at Plataea by the hands of the Gods of the Earth. The play concerns itself only with the depicting of the grief over this catastrophe. The *Persians* is in fact a composition much in the fashion of what we may suppose the *Phoenicians* of Phrynichus to have been, a work framed upon a single motive of despair, expressed by varied personages in varied form, and Aeschylus may with some degree of probability have drawn largely from his most illustrious predecessor.

The *Seven* is undoubtedly the third of a trilogy, according to the statement of the author of the hypothesis, if we might not know it from

common sense when we are aware of the existence of plays called *Laius* and *Oedipus*. From the two words of the *Laius* that are extant, and from the three lines of the *Oedipus*, we can glean nothing as to their mode of treatment; but it is unlikely that a play with the name *Oedipus* related the incidents of the agreement of Polynices and Eteocles about an alternating sovereignty, of the departure of Polynices, of the faithlessness of Eteocles, and of the muster of the army in Argos, — the events which together with the long hovering curse are the immediate occasions of the catastrophe of this play. The improbability that these events were added to the story of *Oedipus*, intricate in itself, is further increased when we consider Aeschylus's manner in the two preceding tragedies of the *Suppliants* and the *Persians*, and in the succeeding work, the *Prometheus*. These plays contain little "action" or "movement" in the modern theatrical sense of the terms, and it is absolutely impossible that Aeschylus should have packed the complete stories of Oedipus and Polynices into one play. The *Seven* depicts the results of the enmity between the two brothers, or, in the final analysis, of the curse upon the house of Laius. A consideration of this simple construction of tragedy would conduct us by an almost infallible line of reasoning to a belief that the *Laius* demonstrated the outcome of the first king's folly, perhaps his death at the cross-roads, or more likely, as in the *Persians*, the announcement of his death at his house and the consequent deeds of Oedipus, or the events centering round the exposure of the child Oedipus. On any conjecture, the drama *Laius* could not have exhibited both the primal sin of Laius, which brought down the curse upon himself and his posterity, the violation of marriage or of the bright deity of Loxias (according as we deem Aeschylus to have represented the curse as coming directly from Apollo or from Hera through Apollo), and at the same time the retribution for this sin in the case of Laius himself. Such a procedure is wholly contrary to the plan of Aeschylus in all the extant tragedies. In none of them does he depict both the crime itself and the punishment of the crime, but only the latter aspect of the history. On these grounds I cannot well see how we can avoid the conclusions that the *Laius* described a certain period of the vengeance of Apollo upon that king; and, granted this conclusion, the assertion is possible that the circumstances which give rise to the whole trilogy, namely, the curse upon the house of Laius, all

precede the time which the three plays cover, and that the trilogy itself deals with the results of these events.

The *Prometheus Bound* offers as difficult a problem. At the outset its position in the trilogy has been much in dispute, though I cannot comprehend any objection to its claim to stand at the beginning. I shall discuss this question in treating the manner of exposition in this drama. Assuming for the present that it held the first position, the fact faces us that Zeus has already overthrown Kronos and has made the compromising marriage with Themis, that the struggle with the Titans is over, that Prometheus has aided Zeus but offended him by the preservation of man, and that the first tragedy, together with the two succeeding, *Prometheus Unbound* and *Prometheus the Fire Bringer*, concerns itself with the temporary punishment for these delinquencies and with the final conciliation. Even if it were necessary to admit that the *Prometheus Bound* is the second of the trilogy, still the aspect of the question would be little altered. Welcker, Schiller, Kviçala, and the rest who would assign to the extant play such a position, conjecture from the name πυρφόρος that the play which they would call the first treated of the stealing of the fire from heaven by Prometheus. They give us only a theory to work upon, but even so the foibles of Zeus would have already occurred, and the action of Prometheus would have to be considered the first step in the threatening retribution. That is, the chastening of Zeus, and not of Prometheus, would seem the main theme of the trilogy. Zeus would become the central figure of the trilogy, the person whose final purpose is righteous, but whose means seem fallacious, and Prometheus would assume the secondary position of a character who, though he is convinced that he is struggling for the right, is in reality the victim of insufficient knowledge of the ultimate ends of the universe. The fact remains that Zeus has already sinned before the action of the play begins, and if *Prometheus the Fire Bringer* were the first of the trilogy, it would only be a case of the catastrophe working itself out a stage earlier than if the extant drama held that position. The *Prometheus Bound* would be the second stage in the vengeance of fate. Thus, on this merely technical question of the usual method of Aeschylus in choosing the moment for the commencement of his play, and upon the position of the extant play in the trilogy, depends in no small degree the intellectual interpretation of the whole

group of dramas. The admission that the *Prometheus* πυρφόρος stood first in the chain goes very far towards forcing us to the more important concession that Aeschylus's thought concerned itself chiefly with the justification of Zeus, and no one knows to what fundamental conclusions upon the calibre of Aeschylus's mind this view of the Prometheus myth would lead us.

In the *Oresteia* the case is perfectly clear and easy. The circumstances that caused the curse on the house of Atreus have already occurred, and the trilogy depicts the havoc of the curse. In the *Agamemnon* not only has the general motive of the curse been sounded, but the particular set of events which gives rise to the first depredations of the curse is presupposed. Iphigenia has been sacrificed, Clytemnestra's wrath has been excited, and she and Aegisthus are living in sin.

Thus, in all the extant plays of Aeschylus the crime has already been committed and the action of the play represents the retribution for the sin. Now, the method of the dramatist of the last four centuries is not usually of this sort, though he may at any time assume such a method. He will rather represent to you the origin of a certain chain of fatality, the workings of this chain, and the result, all in one play. The larger number of lines permissible to him for a single play allows him this indulgence, though, in the case of a trilogy, the Greek dramatist had as much space, but chose deliberately that his space should be devoted only to the disaster itself.[1] Which mode is more advantageous is beside the question, but undoubtedly the choice of the ancient writer gave him greater opportunity for a unity of a certain kind. The fact that Aeschylus formed or followed the ancient tradition is the important point. Such a choice involved him at once in a dilemma of exposition exactly twice as difficult as falls to the lot of the modern playwright. The modern has only to make clear to his audience the relationship of his characters, and the qualities of the characters themselves, while he can

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[1] We need not consider whether the actual time of representation for single plays was the same in antiquity as in modern days. It is certain that the Greek dramatist did not have as much space of lines for development of action as our writers. The slow choral dances may have filled out the time. Nor can we determine whether this smaller allotment influenced the Greek author to exhibit only the final phase of a tragical history, or whether the Greek mind naturally chose this method of lingering over the inevitable workings of Vengeance.

fall back upon the action of the play itself to explain the intricacies in which he purposes to involve them. But Aeschylus and his successors chose such a point in the tale that they had not only to make clear the relationship of the characters and their qualities, but also those very intricacies the results of which, and only the results of which, they believed it was the duty of tragedy to exhibit. Or, to look at it from the point of view of the audience, the Greek dramatist was twice as likely to be unintelligible, and hence had to exert twice the amount of ingenuity in exposition.

For example, compare Shakspere's *Lear* and Aeschylus's *Oresteia*. In *Lear* Shakspere has only to explain in his exposition who the King and his three daughters, Kent and the suitors, and the rest of the personages are, and as the play proceeds, to unfold their characters. But he is utterly rid of the responsibility of declaring any past deeds of theirs, for they have as yet done nothing which will affect the catastrophe of the drama, and he relies on his first act to exhibit by the action itself the division of the kingdom by Lear, the very act which brings on the disaster. So in *Othello*, the marriage of Desdemona and the Moor; and in *Macbeth*, the intercourse with the witches and the murders of Duncan and of Banquo are the direct material occasions of the catastrophe.[1]

Or, in our own day, let us take, for example, *La Tosca* or *Gismonda* by Sardou, or *Cyrano* by Rostand, or, to come to our English theatre, such a work as *The Gay Lord Quex*; and we discover that in all of them

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[1] For it is necessary to remember that it is with material causes that we are dealing, the mere mechanism of construction. The objection could be made with perfect propriety that the real root of the catastrophe in *Macbeth* lies in the innate qualities of the husband and wife. Very well; but mere character cannot exert itself in the abstract to produce a result, but must exert its energy through concrete facts, and it is these concrete facts, these events, with which we have to deal. The characterization is effective in bringing about the dénouement in Aeschylus as well; but the point of difference lies in the fact that the concrete events through which the characterization presents itself occur in Aeschylus before the beginning of the play, in Shakspere and the moderns in the action itself. *Hamlet* smacks somewhat of the Greek type; the murder of the father, which is the primal material force of the drama, has taken place before the period of the action, though, even here, as far as concerns the young prince himself, the immediate occasion of his procedure, his converse with the ghost, finds palpable expression in the first act.

the intrigue itself is first presented on the stage, and then the results, usually only in the last act. What is technically known as the *problem play*, inasmuch as of necessity it treats of a woman with a past, must be tinged by the Grecian method. Pinero's productions are examples to the point. Even here, the direct occasion of the suffering of the woman occurs in the play itself, as, in *The Second Mrs. Tanqueray*, the second marriage of Tanqueray.

In the *Oresteia* the condition is completely reversed, as I have already pointed out. Aeschylus will not consent to the presence in the action even of the immediate occasion of a single play. Nothing occurs in the *Agamemnon* except the beginning of the catastrophe, the home return of the heroes, and the ending, the assassination.

Now, at the same time that the Greek dramatist is coping with the two-headed dilemma, the exposition of both characters and previous events, he must maintain like any other dramatist an ever advancing action. The movement is rendered all the more difficult by the fact that the field of action is so narrow and unified. The Athenians were intelligent enough to require that there should be a continual progress. The long wailings, which to us seem tedious and unjustifiable delays, appear to have been heard by them with genuine satisfaction and pleasure, and even to have been viewed as a lively device for the advancement of the action. Until the dramatist expounds the situation, the intelligibility of the episodes of the play is jeopardized by the existence of this double uncertainty as to the relation of the characters and as to the past chain of fatality, which has brought down upon the characters the catastrophe which the play is to depict. In estimating Aeschylus's facility in exposition, we must therefore consider not only the problem, which often baffles the modern dramatist, the necessity of maintaining an ever advancing movement in the midst of exposition, but also the fact that Aeschylus had to face exactly twice as complicated an exposition. If his plays are more obscure than modern works, we should thus be twice as ready with our pardon ; if he seems to us to have met and conquered the obstacles which beset the path of the ancient dramatist, we should conscientiously and mathematically double our praise.

It is of small value to assert that the myths upon which the dramas were founded were too well-known to the public to require any elaborate exposition. For this consideration alleviates the complications but little.

There are many different versions of identical portions of the same myth. Each epic poet or dramatist had an individual way of interpreting the simple facts of the story, or even of twisting the facts into correspondence with the interpretation that he himself chose. Thus, in the case of the *Seven*, one version had it that Hera was the originator of the curse, and another that the majesty of the sun-god, Loxias, was insulted by the sight of such pollution. The same subjects were treated again and again by each successive dramatic poet ; and the problem presented itself to each of the feasibility of leaving one's peculiar impress upon the traditional story. Thus in the *Choephoroe* no one before Stesichorus had conceived the idea of making Apollo the inspirer of Orestes's crime, and Aeschylus eagerly seized upon the suggestion. He made an even more important alteration, in emphasizing Clytemnestra's participation in the murder of Agamemnon, and consequently in raising the death of the Queen to an equal significance with the death of her paramour, when in all other accounts, as far as we are able to judge, the slaying of Aegisthus had been the all important factor and the murder of Clytemnestra only a necessary adjunct. With many indistinct remembrances flitting through his brain, the listener stands even in greater peril of misconstruing the development of the plot than if he came with a mind in total ignorance of the tale to be imparted. Confusion is more imminent, and just because there are other versions in existence, the dramatist finds it more impracticable to intimate the particular meaning with which he would imbue the myth. I doubt not that Aeschylus had a perfectly definite conception of the relations between Zeus and Prometheus ; but the possibility of our penetrating this meaning has been befogged by the many treatments of the myth in modern literature, which, becoming known to us before the Aeschylean version, render it more difficult to understand the drama as Aeschylus meant it. The imbroglio is made more serious by the conduct of critics who persist in reading into the work their own *a priori* opinions, until our minds are so dissipated by the bewildering extravagance of the many judgments, that they are no longer capable of taking up the play without prejudice or of searching sanely and honestly for the significance that Aeschylus has really embodied in the lines. So, in the end, if the knowledge that the people already possessed of the myths made any difference at all, instead of giving the authors any advantage, it rather

made it all the more imperative that each dramatist should render unmistakable the manner which he individually chose to pursue.

In addition to the exposition of characters and events, there may have been another mode of explanation necessary, an obligation under which the modern with all his elaborate costume, properties, and scenery does not labor. I refer to the explanation of what the author wished the audience to understand as the stage picture. Nothing definite can be stated in this connection until it is finally decided just to what degree of perfection the mechanical devices had attained in the time of Aeschylus. Even the Elizabethan stage, with its conventional system of placarding the scaffolding, found it convenient to explain even further the theatrical setting. So in the second scene of *Twelfth Night* where Viola and the sea captain enter on the coast. Such an additional exposition was of course not needed to state the fact that the scene represented such and such a locality, but the dramatist was not willing to let the opportunity slip with a mere indication of the place. He would rather conjure up in the minds of his listeners, by means of speech, the embellishments which their physical eyes could not convey to them. He would not have them know only that it was a shore, but he would have them visualize rocks, sand, seawood, waves, and foam, and all the accessories which to-day the craftsman's art makes it unnecessary for the playwright even to mention.

Such were the conditions, I take it, in the time of Aeschylus. There are certainly palpable attempts at a description of costume and scenery beyond what the actual properties of the stage represented. The following are illustrations of this point.

In the *Suppliants* from 180 to 185[1] there is graphic description of the approaching host of the king of Argos, such as we cannot possibly suppose was actually presented to view or hearing. The cloud of dust, the ranks of chariots and horses, and, above all, the general impression that the words convey of a mighty troop of warriors could not be rendered with any majesty either to the eye or ear; yet Aeschylus wishes to maintain an atmosphere of heroism and grandeur and finds an excellent expedient in his language. Of the same character is the description of the arrival of the ships, 710 to 723, a case in which a

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[1] The references are to the lines of Sidgwick's edition in the Oxford Classics.

modern playwright (Clyde Fitch) would rejoice to delight the vision of his audience with the whole marine landscape. But even now our mechanical appliances are apt to be tinged with the ludicrous (witness the frequent accidents to the Rhine maidens in *Das Rheingold*), and Aeschylus, trusting rather to his own power of language and to his genius than to the skill of the stage carpenter, chose a safer method. The Prologue to Shakspere's *Henry V* states well the view that Aeschylus himself probably took of the question :

> " O for a Muse of fire, that would ascend
>   The brightest heaven of invention,
>   A kingdom for a stage, princes to act
>   And monarchs to behold the swelling scene !
>   But pardon, gentles all,
>   The flat unraised spirits that have dared
>   On this unworthy scaffold to bring forth
>   So great an object ; can this cockpit hold
>   The vasty fields of France ? or may we cram
>   Within this wooden O the very casques
>   That did affright the air at Agincourt?
>   O, pardon, since a crooked figure may
>   Attest in little place a million ;
>   And let us, ciphers to this great accompt,
>   On your imaginary forces work.
>   Piece out our imperfections with your thoughts ;
>   Into a thousand parts divide one man,
>   And make imaginary puissance ;
>   Think, when we talk of horses, that you see them
>   Printing their proud hoof i' the receiving earth ;
>   For 'tis your thoughts that now must deck our kings,
>   Carry them here and there ; jumping o'er times,
>   Turning the accomplishment of many years
>   Into an hour glass."

In the *Suppliants* there is a continual reference to the statues and altar which must be supposed to stand on the stage, and secondly to the dress and headgear of the women, especially to the suppliant boughs

which they bear. The frequency of this mention is remarkable. The whole matter raises another question, at the solution of which we can again only guess, namely, how far it was necessary for Aeschylus to emphasize continually the very properties which he employed, on account of their meagreness and the consequent likelihood that they would not be understood. It is only another aspect of the general question about his indication of scenery by the medium of words. At lines 20–21, at 190–2, at 234–245, at 334–5, at 345–6, and frequently through the whole play, we find such references to the branches, the sign of the suppliants. Lines 234–245 comprise the King's first speech, where the whole emphasis from beginning to end is on the suppliant boughs. It is the same with the altar and the statues: cf. lines 189, 220–1, 345–6, 893, 859–860.

The descriptions of the odors and rushings that precede and accompany the parodos of the daughters of Oceanus in the *Prometheus*, and of the sublimity of the swallowing up of Prometheus at the end, are instances of an achievement in word painting, of which the handicraft of scenery and properties was incapable.

In the *Seven*, the general temperance of this mode of treatment is brought out in happy contrast to the sensationalism of Euripides in the *Phoenissae*. In the latter play, Euripides causes Antigone to ascend a ladder undignifiedly to the roof, and thence to view the marshalling of the hosts, and to discuss it with her attendant slave. Aeschylus, to whom such a procedure on the part of a highborn lady would have properly seemed ludicrous and unfitting, puts his version of the appearances of the champions into the mouth of a messenger, who has employed the ordinary means of conveyance to enter upon the scene of action; that is, instead of resorting to devices of climbing heroines to obtain dramatic tension for his descriptions, Aeschylus trusts all the effect of his account to the power of words. It is impossible and undesirable to probe for all the reasons that influenced an author in arranging his action in a certain system; but I think that we can safely assert that it was not only a desire to avoid the presence of carnage upon the actual scene which led Aeschylus to plan the whole play so that everything should be told rather than appear, but that also he realized how the poverty of stage machinery would belittle the great scenes which he wished the audience to visualize.

Again in the *Agamemnon* the great word picture of the three illumi-nations at the beginning, the light of the stars, the beacon signals, and the fires of the sacrifices, rises far above the domain of theatrical resources, if indeed we are to believe that the lights were represented at all. The preparations for Agamemnon's entrance into his home, where the tapestries were very probably exhibited, are also apposite ; but here the recurring references to the richness of the embroidery (lines 910, 919–924, 926, 936, 946, 948, 957) are indicative of a necessity to explain to his audience what doubtless some of them could not discern by reason of the size of the theatre. I do not mean that the emphasis on the purple has not its higher purpose in hinting at the blood which is soon to flow ; but to comprehend this hint the people must be made aware that the carpets *were* purple, and that they *were* rich to typify the high quality of the blood. The case in hand illustrates the happy habit of Aeschylus in combining, by felicitous arrangement and choice of words, a double purpose, a characteristic which I shall mention in treating the manner of his exposition. Of importance in this connection is the lengthy exposition by the priestess of her function, in the Prologue to the *Eumenides*.

A last point to be observed, before studying each play separately in the development of the art of Aeschylus, is the difficulty that beset him in the delineation of character. At the present day it is the established precedent that one of the main features of a dramatic production shall be its characterization. This is one of the first qualities that the critic looks for when he is making his estimate of the play. No matter how cleverly the intricacies of the plot are handled, if the actors in this plot are mere puppets in the dramatist's hand, we consider the play a pitiable failure. The stress that we lay upon realistic characterization is still on the increase. The more the drama is studied, the greater becomes the search for delicate touches in this quality, until we grow so eager that we read into the lines what we are so anxious to discover. This tendency puts us in the real peril of underrating the powers of Aeschylus in characterization. We take it for granted that the same conceptions of the value of delineation existed in his day as in ours. But this assumption is unfounded. Tragedy had just started on the road to its high destiny when Aeschylus began to write ; it was still in its formative state when he received it from Choerilus, Pratinas, and

Phrynichus, and it was his genius that molded it into a definite, traditional model.

The old myths, of which the dramatist made use, had stamped certain conventional characteristics upon its heroes and heroines. Analogous examples, which come closer to our own time, are the mediaeval chivalrous romances, the legends of the saints, and fabliaux. These depended for their interest wholly upon the excitement of the story. The personages of the romances were mere stock figures, possessing the same conventional bundle of qualities through all the many romances, and employed by the authors like padded dummies to be twisted through whatever contortions the complications of the tale should render necessary. When a writer appears, who, disencumbering himself of such formalities, can make living human beings out of his personages, we must deal him out not only the ordinary meed of praise for the truthfulness of his delineation, but we must double the measure, because he has not only been faithful to nature, but he has attained to this faithfulness without following any precedent. He has rather evoked it out of his own intellect by his own genius, has freed himself from the bonds of centuries, and has actually created a new literary medium. Of such esteem Chaucer is worthy, and as far as characterization goes, we may say that the works of Aeschylus are also illustrations of the quality. Chaucer's *Knight's Tale* is a chivalrous romance written in the ordinary style of his time with serious intent, but the tale of *Sir Thopas* is a parody on this class of writing, and in such tales as those of the Prioress, the Man of Law, and the Pardoner, a new life is put into the old saints' legends and exemplars by means of truthful delineation of character.

A like development is traceable in the career of Aeschylus. He had to cope with the same sort of stock characters, presented to him in the myths. Upon these he had to bestow fresh vitality, by changing the lay figures into personalities of real life. The additional difficulty fell upon him, not of making a personage out of whole cloth from his own experience, but of giving significance to the traditional types that the myths offered. The case is once more analogous to the gradual emancipation of Italian art from Byzantine precedent through the advances of Cimabue and Giotto and their successors. As I have already noted in general, so now in particular, Aeschylus is doubly praiseworthy because he gave life to his characters and achieved this triumph in the face of

convention. The gradual attainment of excellence in this quality is evident in development, I think, from play to play, and it is to the development of his art that I now wish to proceed.   In forming any judgment upon the dramatic ability of Aeschylus, the three preliminary suggestions that have been made, I think, must always be kept in mind as a foundation : first, the double labor of an exposition of both the relationship of characters and the complication of the plot which pre-cedes the opening of the play ; second, the need of a more specific exposition ; and third, the increased difficulty that he encountered in the matter of delineation of character.   In following the development through the different dramas, I trust that there will appear Aeschylus's special study in one place of one department of his subject, and in another, of another, until in the last of the extant works there is mani-fest a mature mastery of all the secrets of the art.

   To come straight to the point, in the *Suppliants* I believe that Aeschylus is paying especial attention to exposition and manipulation of dialogue, while he has not yet attained to any high degree of excel-lence in characterization.   In some parts of the play the dialogue is as brilliant as will be found anywhere through the whole range of his extant writings.   The exposition is so skilfully managed that nowhere except in the parodos does he frankly admit that he is making clear the matters which it is necessary for the audience to know ; and often a very clever piece of exposition is imbedded in a passage of dialogue which in itself is remarkable for its wit.

   The exposition of the peculiar affliction of the daughters of Danaus and of the proprieties of their conduct occurs at the opening of the play in three places.   First, in the parodos, the chorus of maidens, as I have noted above, frankly address the audience with the tale of their sufferings.   Second, in the conversation with their father, the situation is further elucidated, here perfectly naturally without destruction of dramatic illusion, for it is in keeping that the daughters should discuss their calamity and future plans with their father.   Third, there is a most brilliant manipulation of exposition in the parley with the King.   The King is naturally very anxious to know who this suppliant troop of fifty maidens are, and why they are wringing their hands and beating their breasts through the vales of Argos.   The anxiety gives rise logically enough to a series of excited and specific questions, by which the

dramatist has given himself the opportunity of a minute exposition. It is one of the cleverest devices for exposition that I can recall. At lines 247-8, the chorus, asking the King how they should address him, as private citizen or as ruler, evoke from him a speech (249-273) which makes his station perfectly clear to the audience. The occasionally interrupted stichomythy from 295-347 is especially remarkable. At 295, the King uses the negative $\mu\acute{\eta}$ with his interrogation, — 'There is no story, is there, that Zeus once mingled with a mortal?' — asking the question as if he expected that the maidens, who are being cross questioned, would answer no; for, desiring to make perfectly sure that they are not deceiving him and are in reality Argive women descended from Io, he puts the question so that if they were false, they would be sure to fall into the trap and answer in the negative as he wishes. But they are not to be caught. At 300 again, the question with $o\check{v}\kappa o v v$ is again put as improbable to mislead them. The cross questioning proceeds with all naturalness, affording a very definite exposition of the previous history of the house of Io and of the claims of the virgins upon the mercy of the King of Argos. At 312 and 316 Sidgwick, following Stanley, believes that two lines have dropped out and so punctuates, because he thinks it impossible that the King should himself tell any of the story of Io. But the emendation seems to me wholly gratuitous. It is the only natural thing for the King now to do, to join in and tell the tale of Io. The correctness of the maidens' answers must by this time have convinced him, and so he impulsively helps them in the narration. The fact that the stichomythy is interrupted does not lessen the likelihood of this interpretation, for it is interrupted again in a few lines. On the contrary, such a long piece of stichomythy is unnatural in Aeschylus, and it would be hard to find a parallel. At 318, the King is a little afraid that he may have been too quick in giving credit to their answers, and returns to the method of direct questioning. In the same way at 325, $\delta o\kappa\epsilon\hat{\iota}\tau\epsilon$ shows that he is convinced, but he still continues with his specific interrogations, a fact which proves that such a form of expression is not necessarily indicative of incredulity. There is no chance for tediousness of exposition here; but Aeschylus has made "assurance doubly sure," by varying the expression in every conceivable way, using now in the cross questioning the imperative, now the plain question, and varying these forms themselves with great skill. Further

examples of this variation of the monotony of exposition are the enliven-
ing figures of the turtle doves and the falcon at 223–226, and at 250–
273 the appeal to geographic interest in the second speech of the King,
wherein he reveals his position and the state of the land of Argos.[1]

To dwell now more especially upon the brilliancy of the dialogue, I
leave the references I have made in treating the exposition in the open-
ing of the play to speak for themselves, calling particular notice to the
marvellous acumen with which both the King and the suppliant maidens
urge their respective claims. As the dialogue proceeds from speech to
speech, first the King rebuts the arguments of the maidens, and they in
turn refute his rebuttal until the final conclusion is reached that he shall
refer the whole question to the will of his people. Furthermore, we
have an important instance of the chorus's power of persuasion, where
they turn the King's assumption of inability to their own purpose
(514–515):

> Βα. ἀεὶ δ᾽ ἀνάκτων[2] ἐστὶ δεῖμ᾽ ἐξαίσιον.
> Χο. σὺ καὶ λέγων εὔφραινε καὶ πράσσων φρένα.

So from lines 916–951, the different modes in which the King and the
Herald look upon the Argive duties and the retorts at 948–9 and 950–1
afford a very striking example of the unflagging vigor and energy of the
dialogue of Aeschylus.

The general construction of the *Suppliants* is as carefully studied and
as severely correct as we shall find it in any of the other dramas, and
the movement proceeds in a more direct and uninterrupted line than in
many of the later works. At the end of each episode there has been a
distinct advance in the situation. In some of the other plays, which
are greater in other respects, Aeschylus seems momentarily to have
forgotten this fundamental necessity. Thus, at the end of the first
episode of the *Seven*, no further material change in the conditions has
been arrived at than was understood at the end of the Prologue, though
the frightened conversation of the women with Eteocles and the con-
stant reference to the turmoil of battle may have served to increase the
atmosphere and dramatic tension. Thus again, even in the *Agamemnon*,

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[1] References to geography play an important part in the works of Aeschylus, and I
will state in another place what seems to me their significance.

[2] I keep here the reading of the codices.

lines 617–781, the Menelaus episode adds nothing to the bare plot of the play, and, as far as I can see, little to the general atmosphere.   As in the case of geographical references, I believe that this is another instance where Aeschylus has been carried away by his own desires.[1] In the *Suppliants* the advance is unbroken.   The parodos has explained the perplexity of the daughters of Danaus, and at the end of the first episode the action has proceeded to a point where the King has given evidence of his irresponsibility on the question of their safety, their future vicissitudes are still undecided, and Danaus, the father, has gone to lay the suppliant boughs in different parts of the city at the feet of the statues of the gods.   The short second episode develops the action to the point where the Argives have decided in favor of the maidens. The third episode declares that the enemy are on their tracks, and the division of the dialogue in the exodos makes real the presence of the Egyptians by introducing their Herald upon the stage and by instituting a dramatic struggle in words between him and the King concerning the righteous destiny of the maidens, while the lyric division contains the wailing so dear to the Greek sense of propriety.   The last episode, which emphasizes the determination of the Argives, and the exodos, which makes clear the stern purpose of the Egyptians, leaves the audience in suspended ignorance of who shall prove the victor in the next play of the trilogy.

As far as the development of the story goes, the words of the chorus in the choruses do not form an integral part of the play, except in the parodos, and even here I think that previous conditions could be comprehended by the audience through the exposition in the conversations with Danaus and the King.   But let us grant that the words of the parodos are technically necessary.   The first stasimon repeats the story of Io which the first episode has already told, and only at the last does it approach anything like a direct reference to the present action, in the manner in which it arouses hope for the Suppliants.   Zeus at the last showed mercy upon Io, he is good to our race, he is the wise counsellor and will judge wisely for us, he is all powerful and we fly to him for help.   Such is the connection of this lyric with the development of the plot.   The second stasimon consists of a long prayer for the Argives,

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[1] I will take up the whole question at the end.

who, as Danaus has told them in the second episode, have decided in favor of the Suppliants. Again only at the last does it attain to a very definite reference to present action. The third stasimon consists of cries of distress in fear of the approaching enemy, of whom, in the third episode, Danaus again has given them knowledge. I have already mentioned the wailings with which the chorus march out of the orchestra. In the last strophe and antistrophe there is another last reference to Io, when the virgins beseech Zeus to guard them as he guarded her ; and thus keeping in mind the splendid tale of her sufferings in the first stasimon, we find that her connection with the Suppliants is not only one of ancient ancestry, but also of parallel vicissitudes. Therefore, we can generalize, stating that the stasima of the play form no integral division as far as pertains to revealing the plot. As to the parodos, it is a question. We shall find the same conditions in all the rest of the plays of Aeschylus.

But this is far from saying that the choral passages are unnecessary. They belong to the inmost essence of Greek drama. Without them Greek tragedy could not exist, nor could or would we imagine Greek tragedy without them. They are part of its origin. When we are criticising Greek drama, we must never forget that it is Greek drama that we are criticising, and that in nature it differs fundamentally from English standards. When we say that the choral passages are not technically necessary to the development of the plot, there is no condemnation implied. The same characteristic is found in all the preserved works of the other dramatists. It might be expected that Aeschylus, appearing so early in the history of Greek drama, would have laid some importance upon the choruses as means to tell his tale. He was not so far from the dithyramb, where the tale had been told in lyric passages, that we should surmise that he could have transferred all the important development to the iambic dialogue. But so it is, unless we say that the parodos of the *Suppliants* is a necessary portion, in which case these opening strophes will be a reminiscence of the days when the whole story was sung. In none of the following plays shall we find the words in the lyrical passages of the chorus forming an integral division.

We may go even further and say that in none of the dramas, except the *Suppliants* and the *Eumenides*, is the chorus itself, as a body of

personages, necessary. Of these two exceptions, in the first the chorus are the principal actors, in the second they are of parallel importance with Orestes. In both cases the respective dramas are named from the choruses. It is an interesting fact that at the end of his career Aeschylus returned to a device which he had employed in the very first of his extant works. This latter phenomenon, according to which the body of men, called the chorus, is not necessary to the action, is not so extraordinary as the other, that they do not tell the tale of the heroes and heroines; for in the dithyrambs at first the duty of the chorus had only been to sing the story and not to assume the characters.

There are a number of less conspicuous elements in the *Suppliants*. From 455–467 there is a splendid use of dramatic vagueness, a characteristic which is to be of so signal importance in the *Agamemnon* and the whole *Oresteia*. The virgins intimate their suicide completely by suggestion : 'We have belts and girdles to catch up our robes' (457), and 'to decorate these statues of the gods with new trophies' (463) are especially sinister.

Once in the play we find Aeschylus put at a disadvantage by the limitation to only two actors. In order that the Herald and the King may be on the stage at the same time, it is necessary that Danaus leave, and so at 775 he makes a conventional excuse of going once more to plead with the Argives, who have already promised their aid. It would be much more impressive to have the father of the Danaids on the stage when their fate was being discussed by the Herald and the King.

The only attempt at any careful delineation of character is in the person of the King. The other personages say little more than the myth requires. But on the King's character hangs the fate of the virgin Suppliants. I have already mentioned his conduct, in treating the exposition at the first of the play. He declines to assume the responsibility of bringing war upon his native state. At the beginning he wavers a bit. His nature seems to be of the sturdy type, which it is hard to convince (cf. the long cross-questioning, 234–347), but when convinced he is firm in his purpose (cf. the hot repartee with the Herald, 910–953). He is a true tragic character, wavering between two duties, the duty of compassion and hospitality and the duty towards his country. He chooses wisely to allow his people to decide what is righteous for them.

By emphasizing thus the power of the Argive people to determine the fate of the Suppliants, Aeschylus gives a dramatic tension to the incident of the arrival of the Danaids so that he can justly stretch out this slight event over the space required for a play without suffering the interest to flag for a minute ; and by bestowing a definite personality upon the King, he molds the production into a sincere tragedy, wherein a man struggles between two paths, each of which in the beginning seems to him to offer its own measure of equity. This contest of the King with himself is only the germ of such mighty conflicts as that of Prometheus, suspended between his compassion for mortals and his knowledge of the final justice of Zeus's decrees, or of Orestes, hesitating between his love for his mother and his obligation to his dead father.

To summarize, in the *Suppliants* there is a plain attempt to study seriously exposition, dialogue, and dramatic movement. Less stress is laid upon characterization. There are evident the first inklings of some qualities which Aeschylus is to develop later, but especially remarkable is the manner in which, having absorbed the myth into his imagination, he has molded it into a true tragedy.

The *Persians* is an altogether inferior production, as far as pertains to the aspects which we are discussing. There is evidence of hackwork, and perhaps of imitation, if we are to believe that there was any relation between it and the *Phoenicians* of Phrynichus, as Croiset intimates. It is important in the treatment of the development of Aeschylus's art, principally because of the character study of Atossa.

In the first place the dramatic illusion is sacrificed for the sake of viewing the victory at Salamis from the Greek standpoint. The Persians in their wailings lament their misfortunes in speeches that are formed with only too palpable a desire to arouse the patriotism of his audience. The whole atmosphere of the play in this respect is false. The Persians would be more likely to vituperate their victors, but instead the walls of the palace ring with no disguised glorification of Greek prowess and loyalty. Thus at 134–139 the passage is tinged with contempt for the low passions of the Persian ladies. Again, from 81–92 of this same lyric division, there is a scornful ring in the description of the insolence and mighty preparations of the army of Xerxes, and the elaboration is plainly made for the sake of rendering the more marvellous the success of the Greeks. The Greek idea of too great

prosperity bringing down the vengeance of Heaven also reëchoes through the lines. The use of the word βάρβαρος to describe the Persians (187, 635, etc.) is significant. The case is analogous, though not quite the same, to the retention of the 'barbari' by Plautus in his translations, referring to the Romans themselves. One has only to read the play to feel the pervading impression of Greek supremacy. Every cry of anguish on the part of the Persians means a thrill of joy to the Athenian heart, and Aeschylus is quick to take advantage and to color each lament with a bit of unnecessary grief which finds expression in a woful description of some particular excellence of the victorious Greeks (cf. 349). Such padding, of course, makes the work redundant, as far as pertains to the mere technical expression of the plot. It is necessary also to observe the messenger's speeches at 337 and 353, where he absolutely steps out of his character in order to extol the Hellenic prowess. The whole stichomythy from 232 to 245 is wrong as an element of structure. It is very clearly inserted to inspire every Athenian with a pride in his native city and his countrymen. Atossa would already have informed herself of the sort of city against which her son was to engage in warfare, if she ever was going to take any interest, and though it sounds like a frank piece of exposition, the passage is even thus an unessential exposition, which Aeschylus probably introduced to tickle the ears of his audience. The same kind of fallacy is discernible in the *Oedipus King* of Sophocles, where it is wholly impossible that Oedipus should have lived so long with Iocasta without learning the story of her former husband. But there the tale of Laius is related as an integral part in the development of the plot. To refer again to Chaucer, in the Pardoner's Tale and in the Wife of Bath's Prologue, he is so carried away by his theme that he allows the dramatic illusion to slip and both the Pardoner and Wife of Bath bravely and earnestly to paint their own vices in the blackest colors in the face of the world. So Aeschylus is overcome by his patriotism, until he lets the Orientals echo such praise of the Athenians as scarcely a real Athenian personage in a drama could frame.

As a piece of dramatic construction the *Persians* falls pitiably below the standard set by the *Suppliants*. After the first episode there is practically no further movement. The messenger has told the tale of the disaster, and the chorus have already made a considerable lamenta-

tion. Dramatically considered, there is no further reason for continuing
the play, except the slight suspense that awaits the arrival of Xerxes
. and bridges over the interval between the end of the first episode and
his arrival in the exodos. The second episode, in which Atossa declares
her intention of summoning the shade of Darius, is of absolutely no
importance in the revelation of the plot. It is as if Aeschylus had
added it gratuitously, because he had somewhat habituated himself to a
short second episode as in the *Suppliants* and knew of no other matter
to fill it. The third episode deals with the appearance of the shade of
Darius. It is again necessary to admit that his words contribute but
very little to the advance of the plot. There seem to be two slight
links which bind him to the action of the play. By the first, a link
relating to the past, Darius is connected inasmuch as Xerxes has gone
to Greece in emulation of his father and to atone for the disaster at
Marathon. This is clearly of very remote importance to the *Persians*.
The second link, dealing with the future, is of more significance, in that
from 800–842, by prophesying still further calamities for his son and
for his countrymen, he somewhat increases the dramatic tension which
precedes the entrance of Xerxes.

There is another flaw in construction in that the episodes dealing
with Darius do not have a logical origin in the preceding episode, nor
does the exodos, which depicts the return of Xerxes, arise from the
episodes of Darius, though it has some connection with the report of
the messenger. In the *Suppliants*, each episode is the natural outcome
of the preceding. The only way in which the *Persians* can be accounted
for as a play, is to say that Aeschylus has here employed the method of
heaping one aspect of the disaster upon another until the climax is
reached at the arrival of Xerxes and the consequent cries of anguish at
his plight. Then each incident will have a proper place as a constituent
of the heap of elements which goes to make up the doleful tragedy.
Thus only can it be considered as a unit. But we must go even a step
further and say that it is not only an accumulation of sorrow, but an
accumulation of sorrow wholly from the Greek standpoint. With this
thought the use of Darius becomes intelligible. He is the voice from
the dead, adding his portion to the general denunciation of the Persians
and the glorification of the Greeks, as the messenger before echoed the
opinions of the living. From 790–792 he prophesies that there shall

be no further expeditions of his countrymen against Greece, and pro-
ceeds immediately to detail the extent of the past calamity.   At
809–812 he even blames the invaders for desecrating the shrines of
the foreign deities.   His whole attitude is that of partisanship for the
Hellenic cause, and his appearance thus forms an integral part of the
method of accumulation.

If we can admit that Aeschylus had this method in mind when he
wrote the *Persians*, the composition does not seem so careless.   Even
so, there are many faults.   There is an unnecessary repetition of the
details of the disaster when the shade of Darius appears, and the general
plan would have been comprehended by few of his audience and would
have seemed a very loose aggregation of facts, had it not inspired their
patriotism.

The character study of Atossa is of main importance, as a beginning
of that art which was to culminate in the conception of Clytemnestra.
There are a number of very delicate touches upon her personality.   She
is easily alarmed by her dream and as easily pacified by the consolations
of the chorus (cf. 216).[1]   Atossa's silence from 256 to 290 is a splendid
touch, if we may believe that Aeschylus did it consciously.   I cannot
see why the fact that the preceding passage is choral should have pre-
vented her from joining (as in the *Choephoroe* do Orestes and Electra)
with the chorus in the long lyric that divides the first episode.   Her
silence in the moment of her great grief is worthy of a queen, and is
contrasted with the despairing dirges of the old counsellors.   She
herself has been ready enough with expressions of apprehension in
matters of mere foreboding, but now when the crisis is at hand, she
realizes that the time has come to act.   At 290 she explains her silence
by saying that she has been struck dumb by the tale of the misfortunes.
This is one of her reasons, and it is doubtless characteristic of a queen
to keep silence in her woe rather than to bemoan it in the face of her
people.   At 296, τίς οὐ τέθνηκε is indicative of a great longing to
hear that her son is not of the lost, though she is afraid to put directly

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[1] The use of the dream itself and the chorus is very effective, as a foreboding of
the future evils.   It is interesting as an early example of the method that Aeschylus
was to use in the *Choephoroe*, as the employment of the ghost of Darius is a first study
in the device to which he resorts in the *Eumenides* to arouse the sleeping Furies by
the revengeful spirit of Clytemnestra.

the question, τίς τέθνηκε, in euphemistic fear that Xerxes may rather be of this number. She does not mention his name, but the messenger, understanding, answers plainly, and the relief of the queen in the two following lines makes clear what had been her anxiety before.

The coming of Xerxes is in a certain way the climax of the play. Aeschylus makes use of a number of devices to achieve this climax. First, there has been a kind of tragic repetition of phrase throughout, which consists especially in different modes of expressing "the spear-bearing Greeks," and in a continued emphasis on the binding with chains and the lashing of the Hellespont, as in the *Suppliants* the stress laid upon the altars, the statues, and the fillets. We might conclude that the chief moral thought of the play was the vengeance upon mortals for their insolence against Heaven. Certainly the main thought of the *Suppliants* is expressed by the emphasis on the suppliant emblems. The list of names at 302, which has a logical reason in the desire of the nobles to hear news of their friends, and at 956 when the chorus asks for the generals, serves to heighten the climax of woe by another species of tragic repetition. The summit is reached where at 1017 Xerxes, by exhibiting his rent garments and the empty quiver, brings the calamity concretely home, and at 1025 where the gist of the whole drama is embodied in

<center>Ἰάνων λαὸς οὐ φυγαίχμας.</center>

The amount of dramatic force that is crowded into 1016-1025 atones almost for whatever weakness the rest of the work contains.

To summarize, the *Persians* is faulty in construction and dramatic illusion, even if we admit that Aeschylus had a definite system of accumulation in view. There is a commencement of a study of climax, and a very tangible advance in character study over the manipulation of this quality in the *Suppliants*.

Two characteristics of the *Seven against Thebes* are so conspicuous that I proceed straight to them, intending to refer to the minor points afterward.

First is the masterful use of climax in the speeches of the messenger, the answer of Eteocles, and the comments of the chorus, extending through the second episode from 369 to 719. At the very start, though there has been no regular exposition concerning Polynices, the audience knows enough of the myth to be sure that there could be no expedition

of the Seven against Thebes without the presence of the chief promoter.
Furthermore, in the *Oedipus* there had probably been some reference
to the working out of the curse in the death of the brothers at each
other's hands. Every element of the descriptions of the champions at
the seven gates aims toward the final purpose of the combat of the
brothers. At 283 Eteocles had already mentioned the fact that he with
six other heroes would withstand the seven leaders of the Argive host,
and as each pair of contestants is matched, the audience feels the
chains of fate drawing closer and closer about the two rival kinsmen.
The vivid sense of locality in the mention of the Gates adds to this
suspense. The eagerness with which Eteocles at 448 sets Polyphontes
against Capaneus, and again at 474 Megareus against Eteocles, and the
anxiety he exhibits at the report of the champion threatening the next
gate (cf. 451, 480) are important constituents of the climax. The
figures ring out like notes of doom, as one after another the King
dismisses his followers to the battle : τρίτῳ (458), τέταρτος (486),
πέμπτον (526), and πέμπταισι in the next line, ἕκτον (568) and finally,
with greater emphasis than ever, all in one line (631), τὸν ἕβδομον δὴ
τόν τ᾽ ἐφ᾽ ἑβδόμαις πύλαις. Again, in the pitting of champion against
champion, there has been a continual reference to the suiting of man
to man, and this has foreshadowed the strife of the two brothers, whom
the curse has suited one to the other (cf. ἀνὴρ κατ᾽ ἄνδρα (505),
ἐχθρὸς γὰρ ἀνὴρ ἀνδρὶ τῷ ζυστήσεται (509), and at 554, ἀνὴρ ἄκομπος
fittingly matched against the κόμπος of the Argive champion). Further-
more, there is a climax toward the combat of the two brothers in the
order of taking up the champions, by which a progression is made
from the most evil leaders of the besieging host to the best, Partheno-
paeus and Amphiaraus just before the mention of Polynices. This
occurs in 577 in the speech about the prophet warrior. The height of
tension is achieved in this passage in the great vividness which is
attained by the direct quotation, and in the emphasis in this quotation
on the idea of relationship and the wrong with which Amphiaraus taunts
Polynices (cf. 580 to 589). All these different lines converging to the
same point afforded Aeschylus a valuable training in that kind of climax
which he was to employ in the whole structure of the *Agamemnon*.

The second point of paramount importance is the largeness of the
conception combined with a masterful manipulation of the mythical tale

which the old epics offered him. The brothers were both alike guilty, one for breaking the covenant by which he promised to hand over the rule at an end of the year, the other by making war ·upon his native land. But for dramatic purposes such a view of the myth would not suffice. One of them must be molded into a true tragic character, a human being struggling for what he believes to be right. Therefore, Aeschylus chooses to idealize his Eteocles, obscuring wholly the evil side of his character, according to which he had robbed his brother of his proper share in the government, and as a necessary result he darkens the character of Polynices, until we forget almost, before Antigone pleads for him at the last, that he is really fighting for his right, so skilfully has Aeschylus turned the tide of favor against him, in painting him as the blackest of traitors to his native land. Eteocles is consequently the true patriot, striving to drive back the Argive enemy from his city of Thebes.

The Italian poet, Alfieri, in his tragedy of *Polinices*, founded on the same material, chooses on the contrary to idealize the other brother, Polynices, and names the work after him. In his little essay, *Pareri dell' Autore su le Presenti Tragedie*, in the section referring to the *Polinices*, he justifies himself as follows :

"Antiquity lends him (Polynices) a character most similar to that of Eteocles. But between two ferocious tigers there would have been no opportunity for dialogue ; scarcely would they have seen each other, before they would have made a rush and torn each other to pieces. To render them, therefore, fit for the stage and endurable, I have deemed it proper to color their respective hates differently, in order that the play might turn out to be capable of some sort of dramatic tension. My Polynices, therefore, is of a much milder nature than Eteocles ; he loves very fondly his sister, his mother, his wife, his son, and his father-in-law ; he therefore results in being a personality most capable of moving sympathy and compassion. Eteocles, because he loves nothing but the royal power, is represented as most hateful. . . ."

Aeschylus has even a higher reason for idealizing his hero than making it possible for a battle of words between them. He will not descend to the melodramatic device of bringing them together upon the stage, as Euripides does in the *Phoenissae* by a line of most improbable reasoning. He wishes to represent in Eteocles the true

patriot. Unless we comprehend this fact, we shall utterly fail to understand the plan of the play.

The *Seven* is not only the tale of the workings of the curse upon the sons of Oedipus. It is that, too, but woven in with this main motive is the motive of the brave struggle of the city Thebes with an army of Argive invaders. The theme of the city's danger offers a secondary dramatic tension to the chief tension which centres about the destiny of the two brothers. Thus the prologue and the first episode deal wholly with the danger to the city itself, and, if it were not for the secondary purpose of the play, would be a blot upon the construction, in that they do not advance at all the story of Eteocles and Polynices. The first stasimon, too, relates wholly to the destruction of the city. The speech of the messenger from 792–802, in which he first announces the safety of the city and then the fate of the two brothers, is indicative of the double purpose of Aeschylus. Line 826 is also of significance, where the chorus hesitate whether they shall rejoice at the good fortune of their city or bewail the fate of the children of Oedipus.

The fact that the much discussed portion from 1010 to the end is employed by Aeschylus to state his real conception of the problems of the drama, seems to me conclusive evidence that he himself wrote the passage at the time he produced the rest of the play, or added it afterward in fear that the true moral purpose of the *Seven* would be misunderstood. This last section is absolutely necessary to round off the play, for otherwise the seeming injustice to Polynices would be unexplained. Antigone is used to voice the objection that Polynices was seeking his natural right, and she is finally answered by the Herald, who voices the conscience of Aeschylus. At 1054,

$$\pi\alpha\theta\grave{\omega}\nu \; \kappa\alpha\kappa\hat{\omega}\varsigma \; \kappa\alpha\kappa\hat{o}\iota\sigma\iota\nu \; \dot{\alpha}\nu\tau\eta\mu\epsilon\acute{\iota}\beta\epsilon\tau o,$$

Antigone states emphatically the position of the supporters of Polynices. But 1055,

$$\dot{\alpha}\lambda\lambda' \; \epsilon\dot{\iota}\varsigma \; \ddot{\alpha}\pi\alpha\nu\tau\alpha\varsigma \; \dot{\alpha}\nu\theta' \; \dot{\epsilon}\nu\grave{o}\varsigma \; \tau\acute{o}\delta' \; \ddot{\epsilon}\rho\gamma o\nu \; \mathring{\eta}\nu,$$

contains the judgment of Aeschylus and the moral of the play. Eteocles had certainly sinned, and Polynices justly sought vengeance, but he should have obtained that justice by revenging himself on the culprit alone and not by letting slip the dogs of war at his own fatherland. At 1056–7, the words

> ἔρις περαίνει μῦθον ὑστάτη θεῶν.
> ἐγὼ δὲ θάψω τόνδε· μὴ μακρηγόρει

virtually acknowledge the equity of the Herald's position, and 1057 sounds like a stubborn refusal to submit to what has been proved right. The Herald in the last line of the stichomythy (1058)

> ἀλλ' αὐτόβουλος ἴσθ', ἀπεννέπω δ' ἐγώ

so understands her.

In such fashion is the interest of the city linked to the story of the curse of the house of Laius. The happy union of two purposes is typical of the same genius that Shakspere exhibited in the junction of the tales of the King and of the bastard Edmund in *Lear*. Lines 1076–7 state the double aspect of the work:

> καὶ πόλις ἄλλως
> ἄλλοτ' ἐπαινεῖ τὰ δίκαια.

The last strophe, 1078 to end, a song of thanksgiving for the delivery of the city, exemplifies the broad purpose of Aeschylus and his final conception that the individual should repress his anger at his own wrongs for the sake of the community, for the sake of "his neighbors."

Some minor points remain to be noticed. The *Seven* is interesting as the first extant drama to have a regular prologue. In this prologue, in a few lines, a splendid atmosphere of war is developed, which, maintained throughout the whole play, led the ancients to denominate it as bristling with Ares. The references to the preparations on both sides, and the description of the bloody sacrifice of compact are especially effective.

In characterization, Aeschylus has become so interested that he even delineates the chorus carefully in the first episode. The wailing of the chorus seems to have been a tradition of tragedy, and it is a great advance when Aeschylus gives meaning to the conventional form. This he truly does in representing the women as stubbornly unreasonable and unwilling to listen to the wise admonitions of their King. The piece of characterization is perfectly gratuitous, for the plot does not require that the chorus be anything more than the conventional body of commentators. I have already discussed the delineation of Antigone and Eteocles. The stichomythy, especially at 241–263 and at the first debate between Antigone and the Herald, is very brilliant.

A summary of the play may be both brief and truthful if it states that in the *Seven* all the qualities of Aeschylus's art are very near their perfection.

In the other dramas there is little to study except the developed form of what Aeschylus has learned in his earlier works.

For the *Prometheus Bound* it is first necessary to settle the place of the drama in the trilogy as far as can be determined by reference to the constructive qualities. The whole discussion at the start, however, seems to me superfluous. The scholion on 94 plainly says : ἐν γὰρ τῷ πυρφόρῳ τρεῖς μυριάδας φησὶ δεδέσθαι αὐτόν, and I cannot see by what right L. Schiller could twist the meaning so that these words should be a threat of Zeus, or Kviçala change δεδέσθαι into δεδήσεσθαι. But granting these abortions of text and meaning, we are brought to the same conclusion that the extant play held the first place in the trilogy by the elaborate exposition (100–113) of the former action of Prometheus ; and at 199–278, the preceding circumstances are related in such a detailed fashion that we cannot possibly suppose a former play that has already depicted by action to the spectators these same circumstances. Wecklein bases part of his proof as to the place of *Prometheus Bound* upon this consideration.

The masterful manner in which Aeschylus has fashioned the constituents of the old myth to his purpose is one of the most eminent qualities of the work. If one stops and reflects what the Prometheus of the myth was and then what Aeschylus made of him, so that he has been a typical personage for all times and has excited the interest of such diverse poets as Goethe and Shelley, if we think how much Aeschylus has made the mythical figure of Prometheus mean to us psychologically and ethically, we shall start with a more proper basis for our study. In the *Seven* he had manipulated to his own purpose the facts presented to him by the epics, and now with a stronger grasp than ever he takes both a situation and character in hand, so as to cause them both to express the truly tragical figure of a Titan, fighting for the right, as far as his comprehension permits him to see it, but yet in opposition to the more farseeing intentions of Zeus. Yet the mere punishment of Prometheus would not serve him as fit material for a drama, as before the mere woe for the disasters at Salamis had sufficed for the *Persians*. He had now learned the need of a stronger dramatic

tension. This he achieves by making Prometheus the son of Themis, and by providing him from his mother with a secret which threatens the sovereignty of Zeus. Now Aeschylus would not have dared to preach treason against the chief of the gods unless he meant that in the ultimate analysis Zeus was to be shown just. Zeus has erred in impiety in over-throwing his father Kronos, though his final purpose is righteous, for the rule of Zeus means equity in the universe. But Fate requires that even an impiety for the sake of final justice shall be punished, and so Zeus must be placed in temporal apprehension. Thus the chief of the gods, also, suspended between two ends of fate, is a proper subject for tragedy, and Aeschylus has attained to a summit of dramatic art by so ordering the mythical material that not only has he provided a double dramatic tension, but has so intertwined the respective interests that each is inseparably linked with the other. A discussion of the philo-sophical purpose of Aeschylus is beyond the scope of this paper except in so far as it pertains to the construction of the play; but I think that his whole conception finds best expression in the second stasimon, and the dialogue immediately preceding, especially 543–545, when Prome-theus is censured for having built up his confidence on his private judg-ment, and 544–552, where the final right of Zeus is indicated. The fact that here, as well as in the *Persians* from 1016–1025, and in the last hemichorus of the *Seven*, Aeschylus puts his own judgment of his work into the mouth of the chorus, shows how he discovered a definite use for the traditional body when its service as chief actor had become antiquated.

The vague hints at the secret of Prometheus, growing ever more emphatic as the catastrophe approaches, are of use in binding the play into a closer and tenser unit. The first premonition of a calamity over-hanging Zeus is in the prologue in the lyric between Prometheus and the Oceanids, 168–179 and 188–194. Here the reference is just defi-nite enough to spur on the chorus to learn the whole truth, but the following hints are purposely couched in the most intangible terms to maintain the dramatic suspense. So at 260, in the long exposition of the first episode : —

Χο. οὐδ' ἔστιν ἄθλου τέρμα σοι προκείμενον;
Πρ. οὐκ ἄλλο γ' οὐδέν, πλὴν ὅταν κείνῳ δοκῇ.

The curiosity of the chorus is pricked still further by the stichomythy at 515–525, at the end of the second episode. There has thus been a reference to the secret in the Prologue, and in the first and second episodes, and now in the middle of the Io episode, the third, the threat is made as definite as this first play of the trilogy will admit (cf. 755 to the end of the episode). This discussion of the danger impending on Zeus and the whole episode of Io are of much importance for the following play of the trilogy, and bring home the fact that in considering any single play, if it is a member of the trilogy, we must view it as little more than an act of a modern drama. There would be no such strong need for laying stress upon the threatening disaster to Zeus, unless it were to be the motive in the following work of the release of Prometheus. The prophecy of Hermes of the still increased suffering of Prometheus is clearly a preparation for the next play.

The devices which Aeschylus employs to divide the long speeches which embody the revelations of Io and Prometheus are worthy of notice. The words of the chorus at 631–634, wherein they beg first Io to recount her sufferings and then Prometheus to prophesy her future woes, are plainly employed as a means to break up a report which would otherwise be of the same nature as the long conventional announcement of the messenger. But even these divided accounts are varied by a most clever dialogue (742–785), consisting partly of stichomythy. At 752–756, the transition to the coming narration is exceedingly well handled. There are three distinct steps: first, Prometheus asks Io how she could bear such woes as his; second, his woes are unended by death; third, only Zeus, by his fall from supreme power, will release Prometheus. This hint at the fall of the chief of the gods, by exciting Io, prepares the path for the prophecy, which so intimately binds the fate of Zeus, of Prometheus, and of the wandering Io. Again, in 778, the refusal of Prometheus to gratify the woman in both of her requests is only a device by which through the mediation of her chorus, who desire to learn the answer to the other request, Aeschylus seeks again to avoid the tediousness of a lengthy piece of iambic trimeter. Aeschylus's method undergoes a still further development. At 742 Io had interrupted Prometheus with her cries of distress. Aeschylus thought that at this point a continuation of her travels would prove a little monotonous. But he himself and his audience like geography,

and he knows that the map of the wanderings will seem all the larger and will be all the more agreeable, if it is divided. So he interrupts it by dialogue and by a prophecy of her future peregrinations, and does not resume the tale of her former journey until 827, with the very plausible excuse that he must relate the past to prove the truth of what he has prophesied for the future. It is not an impossible suggestion that these devices for the division of the tale of Io found an additional reason in a desire to give to the protagonist the greater number of lines.

The exposition and general construction are clearer and more direct than in any play since the *Suppliants*. The Prologue establishes Prometheus in his station of punishment, the first episode is an exposition of his past, the second episode is a further retailing of his benefits to mankind, with a transitional passage at the end relating to the hope of release, the third episode of Io both makes clear the significance of this hope and serves as a kind of Prologue to the *Prometheus Unbound*. The exodos, which is a unit in itself, in addition to its connection with the rest of the drama, begins with a kind of repetition of what has gone before, a kind of separate episodical exposition, which prepares the way for the depicting of the stubborn fortitude of Prometheus, the topic of the exodos. The catastrophe follows close upon the climax of his obstinacy.

Here again a summary can only state that the art of Aeschylus is at its height. The personality of the young tyrant Zeus overshadows the play, though he nowhere appears. The dialogue is exceptionally brilliant, especially the high language between Hermes and Prometheus at the end, when the vindictiveness of the Titan and his utter hate of the gods reach their climax in a storm of words, that is succeeded directly by the avenging storm of reality. The other qualities of the work I have already treated.

The *Agamemnon* has been so generally praised that it is difficult to say anything about it which will not seem absolutely trite. I shall therefore in this place touch only upon certain salient points which have attracted my attention and which are of special interest in connection with the development of the dramatic art of Aeschylus. The *Oresteia* is his latest extant work. The three dramas were very probably written at the same period of his career, and what may be said of one of them usually finds its parallel in the two others. Thus there will be constant need of a reference back and forth in treating the trilogy.

Of first importance is his great conception of the character of Cly-
temnestra. Upon no other personage in the entire range of his exant
works does Aeschylus outlay such conscious and delicate labor, and
withal appear to take such delight in the delineation. He plainly is
reluctant to relinquish her, even after her death in the *Choephoroe*, and
he must needs introduce her distracted ghost in the *Eumenides*. Not
that a fondness for Clytemnestra is the only reason for her appearance
there; she is employed to arouse the Furies to their task. But even
so, she is not indispensable, for there is no inherent reason that the
chorus should not awake by its own initiative. Therefore, as far as
the mere framework of the piece goes, Clytemnestra is not needed, but
no one will doubt that her presence as the vindictive genius of Orestes
is a device of inestimably greater dramatic power, and, as the introduc-
tion of Darius in the *Persians*, so here the shade of Clytemnestra, link-
ing human action more vividly with the terrors of the underworld, serves
to enhance considerably the atmosphere of unrelinquished gloom and
fright. I cannot think, however, that this consideration alone induced
Aeschylus to bring her upon the scene. His lurking fondness for this
creation of his own imagination must have influenced him to retain her
even to the last.

Her personality certainly dominates the action from the very first.
At line 83 of the *Agamemnon* the chorus address her, and from that
time until 257 she is either wholly engaged in giving orders for the
elaborate sacrifices of thanksgiving, or we may suppose her partly thus
engrossed, as if to dull her nervous apprehension, and partly standing
aside in silent revery. Our first glimpse of her character is that of a
woman of wordly experience, hardened, incredulous, attributing trust in
dreams to slumbering souls (275), and belief in rumor to unsophisticated
maidens (277). But these are merely her words; and when we are
brought face to face with her excessive alarm at her dreadful dream in
the *Choephoroe*, we have a choice between two lines of explanation.
Either the long years of suffering have broken her brazen spirit by the
time that the action of the *Choephoroe* is supposed to take place, or
she is seeking at these lines (275–277) to console herself by pretending
that she puts no faith in visions or reports. If the last alternative is
correct, it is only spasmodic comfort, for she proceeds immediately to
relate her sure proof of the travelling beacons. The complete virility

of this description of the lights, the sternness and vigor of the language, the masculine characteristics which the invention of such a scheme of signals implies, appear to me emphatic evidence that we must choose the other alternative of the breaking of Clytemnestra's spirit after a lapse of many years. Yet there is a touch of sorrow in the very first word that she uses (264) εὐάγγελος in describing the morn, which she knows is fraught with little happiness for her. The following speech (320–350) is very important as revealing the cruelty of her character, for it rings from first to last with exultation at the havoc caused by the victors and at the misery of the captives and those who have lost their loved ones. In the stichomythy from 538 to 550, I can see no reason for dividing the lines between the Herald and the Chorus instead of between the Herald and Clytemnestra, except a desire, wholly unjustified by the text itself, to represent Clytemnestra in another long meditative silence (503–586). The codices give the stichomythy to Clytemnestra. A definite silence, when it would be natural for her to speak anxiously at once, would doubtless be effective, as depicting the Queen planning her mode of attack,[1] but the device would be somewhat unnatural and plainly artificial, and we are certainly not warranted in introducing it arbitrarily into the text. Her elaborate account of her desire to welcome her lord (587–614) absolutely echoes from a hollow insincerity, where the very elaboration is a clear brand of hypocrisy; yet, at 600–603, she makes a special effort to gain the credence of the chorus.

> ὅπως δ' ἄριστα τὸν ἐμὸν αἰδοῖον πόσιν
> σπεύσω πάλιν μολόντα δέξασθαι — τί γὰρ
> γυναικὶ τούτου φέγγος ἥδιον δρακεῖν,
> ἀπὸ στρατείας ἀνδρὶ σώσαντος θεοῦ
> πύλας ἀνοῖξαι; ταῦτ' ἀπάγγειλον πόσει.

Again, at 855, when Clytemnestra begins the long oration of welcome to her husband, the very first lines are blotted by insincerity, in her boast that she is not ashamed to exhibit her love for Agamemnon to

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[1] Mrs. Fiske, in her impersonation of Hedda Gabler, emphasizes this mode of expression twice, once in the second act before Hedda drives Eilert to his ruin, and most strikingly in the last, before she decides upon her own suicide.

the eyes of the multitude (850–1). The series of insincere epithets which she addresses to Agamemnon is noteworthy.

Now, this speech of welcome, in which are related the woes of the wife who waits and watches for her husband, is the natural thing on the lips of any woman who is soliciting the admiration of her lord for constancy. But Clytemnestra is a little bit too effusive ; and the words secondarily serve to emphasize her actual infidelity. Thirdly (and this is the point I wish to mark most clearly), I believe that the speech is a real effort on the part of Clytemnestra to excuse herself, not to Agamemnon (for he does not know), but to the chorus, who, in their knowledge of her guilt, would understand well enough that she meant that her weak woman's nature had not been firm enough to stand alone without a man by her side, and so she had taken Aegisthus to herself in lieu of the absent Agamemnon. Besides, had there not been many reports (869) that her lord had been slain? There is truly an element of pathos in her words, the words of a woman who finds herself forced to dissemble, but yet would at the same moment extenuate her fault. This pathos has not been lacking before (cf. 601), where there is surely a note of regret that she cannot joyfully greet her husband at the door.

At 904–5, πολλὰ γὰρ τὰ πρὶν κακὰ ἠνειχόμεσθα, there may be a hint at the evils which not only followed Agamemnon's departure, but also those which preceded, that is, the destruction of Iphigenia. Yet Clytemnestra does not dare to be more definite. A lesser dramatist would have made her reveal at once her savage fondness for Iphigenia, love as of a tigress for her offspring, and the ever smouldering rancour for the maiden's murder and the desire for vengeance, but Aeschylus is more clever, and his Clytemnestra does not speak definitely of her daughter until there is no longer any chance that her web of treachery shall fail to ensnare.

The presence of Cassandra is the last dram to weigh down the balance for Agamemnon's assassination and to make the Queen resolute in her design. At 1438, when the murder has already been committed, Clytemnestra urges her jealousy as another reason for her crime, in a speech (1431–1447) which is marvellous for its remorse and for its utter despair, as she catches frantically at one straw excuse after the other in hope to grasp some beam that shall raise her from the sea of blood in which she has sunk herself. Following a passage (1412–1425)

in which she at last lets flow her long pent feelings in a bitter outburst of grief for Iphigenia, these references to Cassandra have the cumulative effect of a second excuse. As a fourth excuse at 1500–1504, she urges the thought that it was the curse resting upon the house that did the deed, not she, and at 1523 she again defends herself with a reference to Iphigenia. Aeschylus himself seems anxious to excuse her, and Clytemnestra herself is absolutely pitiable, as she wavers between her different pretexts. Add to these considerations her sad attempt (1372–1398) to seem to glory in the assassination and its gruesome details, which, half sincere, still retains a kind of fiendish glee in the vengeance upon the man who has wronged her, and the desolate passion with which she clings to Aegisthus at 1434 ff., and we have a spendid example of that subtle power which arouses compassion for Claudius, the uncle of Hamlet, even in the midst of his crimes, or for Lady Macbeth as she walks at night in her delirium of repentance. We shall utterly fail to comprehend the complete character of Clytemnestra unless we take into account this constant desire to excuse herself; and it is a remarkable confirmation of the truth of this conception when we see the same thoughts continued by Aeschylus into the *Choephoroe*, where at the moment of her death, one after the other, Clytemnestra in her terror alleges these identical excuses: at 910, that it was Fate, not she, who had committed the crime; at 918, that if she herself was frail, Agamemnon also had sinned; and at 920, that it is hard for a woman to be long deprived of a mate.

The conception of Clytemnestra as worthy rather of pity than of hate dominates the *Choephoroe*. The new aspect of her character is emphasized more and more as the *Agamemnon* draws to a close, and at 1660 and 1672–3, she seeks to exert a calming influence, blaming all to the Demon of the Family. She is full of excuses. At 1040–1045 she even excuses her conduct toward Cassandra. At the very beginning of the *Choephoroe*, we find her still remorseful, seeking now to expiate her villainy by propitiatory sacrifice. She does not appear upon the scene in person until she learns of the death of Orestes, and I can find no reason to doubt her sincerity here. Aeschylus has achieved a consummate task in thus subtly altering the sentiments with which we look upon her, until at last, instead of a woman who is willingly a virago, we see her a wretched ruin of what she had been when first Agamemnon had led her

across his threshold, now kneeling at her son's feet, trembling between her fear for her life and her anxiety to justify herself in the eyes of Orestes. In contemplating the delineation of Clytemnestra, also, we must always keep in mind, even more than in the case of the *Prometheus*, the fact that Aeschylus took her out of the land of the conventional myth and made her the chief character of his trilogy, whereas his epic predecessors had always conceived Aegisthus as the chief sinner, and had considered her wickedness and punishment only as a necessary but subordinate accident of the tale.

In connection with this, his supreme study in delineation of character, it is interesting to examine other minor examples of this quality exhibited at the height of his career. The most conventional of all types, the Herald, in *Agamemnon*, is individualized. The Herald of the *Seven* (a play in which Aeschylus seems to have made a special study of characterization, as exemplified by the gratuitous touches in relation to the timid chorus in the first episode) is somewhat of a boaster of his own prowess. The Herald in the *Agamemnon* is strongly outlined by his love for his native land, and his unrestrained joy at returning. The Herald in the *Persians* (261) mentions the fact that he had not hoped for a return, but this single touch is not to be compared with the definite personality as presented by lines 506–507 of the *Agamemnon*, the φίλαι στέγαι of 518, and by the ensuing stichomythy, especially 539,

Κη. χαίρω γε· τεθνάναι δ' οὐκέτ' ἀντερῶ θεοῖς.

Again, the characterization of the old nurse at the moment of greatest tension in the *Choephoroe* is very significant both of an interest in human nature and of an attention to the lesser things of life. There are other indications that Aeschylus observed the familiar and homely occurrences of everyday life. So in the *Agamemnon* (603), the reference to the wife throwing the door gladly open to greet her spouse, and in the *Suppliants*, the description of the heifer calling the herdsman by its lowing (350 ff.). The nurse is the only really comic character in Aeschylus. The device of her introduction is the same that Shakspere employs when he wishes to relieve, and bring out by contrast, the extreme tragedy of a certain scene. So, the fool in the storm in *Lear*, and the grave-diggers in the fifth act of *Hamlet*. In the *Choephoroe*

the sputtering nurse lulls the terror of the situation for a moment, but when the dire catastrophe does occur, its effect is all the more awful by contrast.

Of like perfection with the study of character is the manipulation of vague hints and forecastings and of symbolic language. At the very first of the *Agamemnon*, line 67, the chorus hints most indefinitely at the approaching disaster. At 103–4 there is an intimation of Clytemnestra's purpose. From 122–158 the story of Iphigenia is sung by the chorus in gloomy sounds and symbolism; lines 248–257, especially 254, contain a reference to the death of the King. At 343 Clytemnestra intimates more clearly her purpose when she dwells on the fact that all the Trojan heroes have not established themselves in safety at their homes. At 370–1, in the first stasimon, there is a hint at the retribution to fall on Clytemnestra in the *Choepheroe*. The Tale of Paris in this stasimon foreshadows in symbolism the vengeance to fall on that other adulterer, Aegisthus. At 437–455, the dirge about the Trojan tombs and the burials leads up to a dark apprehension of what the coming night may conceal. At 547–550 the chorus hint that the dark crimes at home have made them desire the return of the army. Line 612 brings the more definite intimation with χαλκοῦ βαφάς. The old counsellors of Argos fear to tell the King of the sin of his wife, but they disclose this secret as clearly as they may in their address to him at his first entrance. By putting two and two together, he might have guessed the truth; for at 795–8 they bewail a watery friendship, and at 808 they speak of someone who has managed affairs at home wrongly. To this method of building up dramatic expectation by foreshadowings, there is added at the end of Agamemnon's speech the intense irony according to which the King enters his home with the greatest assurance, using words of trust which the audience know are untrue (cf. 848–9, the reference to the healing remedies, at 851, δόμους ἐφεστίους, and at 854, ἐμπέδως μένοι). The following passages assigned to Clytemnestra are so crammed full of phrases of double meaning, are so splendid in their dramatic irony, that an analysis serves only to destroy their power. I mention only instances here and there. At 868 and 872 there are veiled references to the net and the mantle that are to be Agamemnon's shroud. At the end of her first exhortation (912–913), she cloaks her approaching action with language that is perfectly intelligent to the

audience, but the real sense of which the king cannot grasp. It savors too much of sacrilege to dissect the ἔστιν θάλασσα passage. Suffice it to say that Aeschylus's use of symbolism here attains to its height, where the details of the murder are each foreshadowed until the very act is seen before it is committed. But this is not enough, and Aeschylus has conceived the most happy device of causing Cassandra by her gift of second sight, now in no symbolic language, but in straight-forward description, to recount the deed at the very time when it is being performed in the bath, by the robe, by the sword. In a word, we gain the double impression of a future and a present verity.

Aeschylus concentrates all his power about the murder of Agamemnon. The act comes at the end of a series of even clearer hints, prepared for by symbolism and converted into plain description. The mouth of Cassandra is closed until it is impossible for her longer to prevent the assassination. The cruelty of Clytemnestra culminates, when at 1055–1061 with dark intent she cries to Cassandra that the victims will be sacrificed, and asks the sinister question why the foreign prophetess is not a part of the oblation. The identity of lines 1072–3 and 1076–7, and again of 1080–1 and 1085–6 affords an example of tragic repetition, which, as one of the modes of most frantic expression, leads the way directly to the climax. This is reached when Cassandra, ascending the steps, which to her mean the ascent of a scaffold, starts back at the stench of the murder which breathes forth through the door.

Exactly the same method of hints and of concentration of every means of dramatic force about the catastrophe is used in the *Choephoroe*. I name only a few of the instances: lines 108–123 could be easily understood in their real sense by the listeners; at 386–388 there is a very definite reference to the approaching death of both culprits. The long dirge, which impresses us as tedious, must have been the delight of the Greeks, and it further serves the direct technical purpose of creating an atmosphere of vengeance, where the frequency of the idea of blood for blood palliates the matricide of Orestes, and again of screwing up the courage of the son to the "sticking point." By lines 418–422 the references to the approaching assassination have become so definite that Orestes explains why it will be easy for him to kill his mother. The dream of Clytemnestra has hitherto been a threatening cloud, but now just before Orestes advances to his task, it is told and becomes the final

incentive. The second reference to the dream at 522–550, the thought of which has been hovering in the imagination of the audience since its first mention in the parodos, combines all the preceding blackness of language into a warning against the immediately succeeding disaster, about which, as before, Aeschylus has grouped all the means of climax at his command.

The three plays of the series are generally well constructed with a clearly marked plan. I take for example the *Choephoroe*. In the Prologue, Orestes has appeared upon the scene with Pylades; at the end of the long first episode, all the plans for the attack are completed; at the end of the second, Orestes has succeeded in the first part of his plan, so that he has gained admittance to the palace and lies waiting to strike the blow; at the end of the third, both murders have been accomplished, and the exodos represents the onslaught of the Furies in retribution. There is a careful preparation at the end of each of the first two plays for the next. At 1280 in the *Agamemnon* there is a prophecy of the coming of the avenger, the young Orestes; at 1318–19 there is a second allusion to the matter of the *Choephoroe*, and again at 1430. So the idea of the curse is not emphasized until toward the end Cassandra begins her ominous ejaculations, as it is to play a far more important part in the next drama than in the *Agamemnon*. At 1646 and 1667 there is a still further allusion to the coming action, and the whole play ends with the thought of the punishment for which the insolence of Aegisthus cries to Heaven. So at the end of the *Choephoroe* the ever increasing importance laid upon Loxias prepares for the dominating thought of the *Eumenides*. Thus at 985, 1026–8, Orestes excuses himself as under the direction of Loxias, at 1035 the theme of the next drama is suggested, at 1059–60 the chorus hope in Loxias as the deliverer from trouble, and the action ends with an interrogation as to when the curse will have worked its full devastation.

Aeschylus in this latest of his extant works displays an appreciation of mere dramatic scenes as such. So the opening scene of the *Agamemnon*. An atmosphere of mystery is gained at the outset, as in the first scene of *Hamlet*, by the introduction of the lonely and weary sentinel as he watches for the beacons. The constituents of this scene are noteworthy. First, there are three distinct illuminations mentioned, the glimmer of the stars, the blaze of the signal fires, and the glare of

the sacrifices, introduced one after the other until they are all burning
at the same moment.   Visualize in this lurid light the excited watch-
man, the timorous old men, Clytemnestra brooding over the scene like
a spirit of revenge, and the noise and feverishness of the night withal,
and you have an idea of the manifoldness of the stage pictures that
Aeschylus imagined, even if the mechanical appliances were deficient.
The dramatic power of the opening scenes in the two other plays are
also worthy of consideration.   Analysis does not always mean compre-
hension.   There are certain situations which we instinctively feel and
call dramatic, and the pleasure of which is crushed out when we subject
them to the psychologist's examination.   If we shall simply conjure up
in our minds a vision of what these scenes were, I am confident that we
shall most surely grasp their true power.   Think of the *Choephoroe*,
with the captive maidens moving solemnly through the parodos of fear
and woe, Orestes and Pylades lurking anxiously behind in eagerness to
learn the purport of the procession, Electra in the foreground, leading
the band, wavering between the several paths of duty that lie before
her, praying for the return of her brother when he himself stands
watching her; then the final picture of the play, when in the deepening
twilight (660) the two bodies lie exposed at the back of the stage, and
frenzy is just beginning to seize hold of Orestes.   Or in the *Eumenides*
the Pythian atmosphere is gained at the very first by the exposition of
the priestess, and then we have the picture within the temple of the
Furies in ghastly sleep, as if to typify the final calm that is to fall upon
Orestes, and the hallowed presence of Apollo appearing in person to
direct the youth to his place of rest.   By means of the opening picture
Aeschylus initiates the feeling of peace which is to conquer the wildness
of the Furies and at last to prevail.

   To some extent in all his plays, but especially in the *Agamemnon*,
Aeschylus further complicates the difficulties of the double exposition
of persons and events by choosing the mode of gradual elucidation.
This is the mannerism that Ibsen in our day has taken to himself, and
by such procedure demands most careful attention if he is to be com-
prehended.   The usual method of modern playwrights is to consume
the first act with little more than explanation, and we of the audience
have so habituated ourselves to this unreality that the two servants
retailing the history of their masters and mistresses no longer grate

upon us. But Ibsen will not permit this. He wishes his first act for development of the situation, and so he reveals fact after fact only as it is absolutely needed for intelligibility, as the play proceeds. Now, Ibsen has in addition deliberately chosen the Greek method of representing on the stage the catastrophe of a given set of circumstances, and not the circumstances, and thus the same double obligation of exposition lies upon him as upon the Greeks. When the intricacy of spreading this in itself difficult exposition is added, obscurity can only be avoided by the most delicate painstaking.

Such is the mode emphasized by Aeschylus in the *Agamemnon*. He discloses no fact until he must make immediate use of it. He encounters, to be sure, the danger of obscurity, but he also avoids to some extent the disadvantage of bewildering the minds of his audience with a simultaneously announced heap of data, which it is likely that they will waste some time in disentangling. Thus, for example, no definite mention is made of the curse until Cassandra refers to it as responsible for the murders that are being perpetrated. Again, the story of Thyestes is not told until the entrance of Aegisthus, with whom it has specific connection. To cite one parallel in modern writings, Ibsen does not allude to the fire in *Hedda Gabler* until the third act, of which it is to be so important an element.

The period of highest development of most men is marked by a freedom from the conventions which bound them in their youthful works. So we see it in Beethoven, through the last five symphonies, in which he oversteps the strict classical forms of his predecessors ; and so preëminently in this last trilogy of Aeschylus. The thing that everyone is quick to note is the introduction of the third actor. Yet this is no unheralded or instantaneous innovation. It begins with the introduction of mute figures, who, nevertheless, are titled as specific personalities, as, for instance, Bía in the Prologue of the *Prometheus*. A further step is taken when one who has hitherto remained silent speaks a few words, as Pylades at 900 of the *Choephoroe*. Here the meagre three lines assigned to him indicate the chariness with which Aeschylus adopted the third actor. He may have very well been influenced in 458 B.C. to give up the old regime by the innovations of Sophocles, who had already defeated him in 468.

A second departure from his old manner is the abandonment of the unity of place. At 657 of the *Choephoroe* there is a sure change of the locality of the action from the region of the tomb to the palace itself. Again in this play the back of the stage opens to exhibit the two biers. In the *Eumenides* at the very outset there is a transference of the scene from the outside of the temple to the interior, unless we suppose the absurdly impossible performance, that the Furies first rush out of the temple and then sink down to sleep again, only to be awakened at the next moment. This waking of the chorus to their duties is a brilliant innovation in comparison with the traditional entrance in a continued dance. At 235 the change to the temple of Pallas at Athens is indisputable, a device which is wholly radical, as here the transference is from one district of land to another.

There is still further a violation of tradition in the arrangement of the *Eumenides* into two episodes, and from 566 to the end into a long series of alternating choral and dialogue divisions, but with nothing that corresponds to the conventional stasimon, the whole partaking rather of the nature of an irregular and extended syzygy (συζυγία).

Thus, in the *Prometheus* and the *Oresteia* the dramatic art of Aeschylus is represented in a perfection of those aspects which he had been studying in the earlier plays: in the *Suppliants*, exposition, dialogue, and construction; in the *Persians*, the delineation of character; in the *Seven*, the molding of the details of a myth to his own will, climax, and again characterization. I have in addition, in the consideration of the different dramas, sought to suggest minor aspects that do not properly fall under any of these larger headings. There are a few qualifications to be made in conclusion.

We deduce from the preceding examination that Aeschylus, despite his early date, had already mastered the chief principles of his art. But for all that, he often let his own desires run away with what his learning and his cool reason forbade. This failing is preëminently apparent in the long geographical passages.[1] I think we shall be forced to the

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[1] Professor Weir Smyth warns me that the Athenians had as yet given little thought to the extension of a sea empire. He also thinks that emphasis should rather be laid upon the general impulse of the times toward geography than upon any conscious tendency of Aeschylus to cater to the multitude.

conclusion that the Athenians in the first period of their history were inspired by their enterprising souls to learn of the far off lands which some day might acknowledge their sway. Who could know? Had they not driven back the hosts of the Persians, and was not the Aegean theirs? Aeschylus, recognizing this lively interest in geography, and filled with the same patriotic spirit himself, consciously catered to his audience by indulging them and himself in the popular topic of the day. Even in the *Suppliants* (249–273) we see in the King's description of his realms particular attention paid to geography, but yet not so as to offend the taste of the critic of technique. It is a different matter, however, when we come to the Io episode of the *Prometheus Bound*. There is no constructive justification for the continued minuteness of geographical description. So also from fragments 194–199 of the *Prometheus Unbound* we may conjecture that, in depicting the wanderings of Hercules, Aeschylus gave evidence of the same eccentricity. From the standpoint of the geographically unenthusiastic and unbiased critic the padding of the passages which recount the wanderings are tedious and delay the action. The Athenians probably received them with satisfaction, and Aeschylus, in opposition to what he had proved that he knew about correct construction, violates his reason to gratify his own hobby, which luckily for him is the fashion of the day. (Cf. especially *Prom.* 786–818.) So in the *Agamemnon* the elaborate mapping out of the situation of the beacons does not strike us as so ill chosen, because of the pride with which Clytemnestra dwells upon the details of her scheme, and because of the powerful climax in which the geographical series closes.

In the Menelaus episode of this same play Aeschylus is again guilty of a blot upon the general excellent construction that marks his work, when he trifles with the direct movement by the introduction of extraneous matter. I cannot but suspect that he had written down the stasimon which follows the dialogue concerning Menelaus for another unpublished play, or that he liked it so well that he transferred it bodily from another drama as a repetition in this second place. The dialogue from 617 to 681 is plainly lugged in to justify the truly marvellous piece of poetry of which the second stasimon of the play consists. The whole section from 617–681 hangs to the main body of the drama only by the slightest thread, according to which, at 662–4, the Herald states

that Agamemnon's ship escaped the storm, a fact that is absolutely
unnecessary to the plot, for the return of the King has already been
made sure. There are four reasons, I take it, which induced Aeschylus
to admit the foreign episode. First, it gave him opportunity for the
description of the storm (636–680). Second, it would serve as an
introduction to the strophe about Helen (681–698), which he may have
composed previously, and lines 686–688 of which would especially
delight a Greek :

> τὰν δορίγαμβρον ἀμφινει-
> κῆ θ᾽ Ἑλέναν; ἐπεὶ πρεπόντως
> ἑλένας, ἑλάνδρος, ἑλέπτολις. . . .

Third, it offered an occasion for the figure of the lion cub, beginning at
717 ; and lastly, for the general moralizing about justice and unjustice
which brings the stasimon to a close. Perhaps Aeschylus, finding the
tale in his source, was loath to let it slip, or allowed it to creep in with-
out consciousness of the interruption in the action. When it had once
found its way into verse, perhaps the poet so liked his composition that
he determined not to succumb to any more constructive scruples.

Of like nature are the passages where Aeschylus lets his consciousness
of power in straight narrative lead him astray. Examples of his narra-
tive excellence may be found in the tale of Io in the first stasimon of
the *Suppliants,* and of an almost epic style, the passage in the *Persians*
beginning at 447 (νῆσός τις ἐστί) with the regular epic opening. So
in the *Agamemnon* (184–257), the tale of Iphigenia. In these cases,
again, his narrative expositions often allow him to forget that he is
writing drama instead of an epic, with the result that the passage is
rather a splendid narrative than an intrinsic part of the play.[1]

Last in consideration of the cases where he has yielded to his desire
more than obeyed his reason, is the description of the future Areopagite

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[1] There is a curious analogy here between Aeschylus and Calderon. These same
long, undramatic narrations are found to even a more unfortunate degree in the
Spanish poet. In the case of Calderon these passages were in imitation of the old
romances, the love for which, being deep-rooted in the people, the dramatist naturally
indulged. In Greece, the same myths which Aeschylus treated had already been sung
in epic and dithyrambic recitations, and the Greek dramatist naturally recurred often
to his early models.

council in the *Eumenides* (681–710), and the rest of the play where the interest seems to swerve from Orestes to the city of Athens. It is possible that, as in the *Seven,* he wished to create in the *Eumenides* a double purpose, which should stretch beyond the individual Orestes to the higher plane of common patriotism. If such is the case, the play is faulty in that there is no emphasis laid upon the higher aspect from the very beginning of the work, whereas in the *Seven* the safety of Thebes is emphasized from the first. I suspect that, in proportion as the opportunity for his patriotism to vent itself expanded, the strictness of his constructive principles grew less and his lenience towards his own passion more. His emphasis on Athenian history is based on the same foundation as his peculiar interest in geography. Our judgment of him in these cases will determine whether we are stern critics of technicalities or are willing to sacrifice form for the deeper things of the spirit.

I trust that in this article I have not appeared to belittle the powers of Aeschylus by pointing out the many devices of dramatic art which he esteems it not beneath him to employ. The crucial test of a dramatist finds an answer when it is ascertained whether he has employed theatrical devices as ends in themselves or as stepping stones to loftier achievements. Ibsen (and I have used him often as an example, because I think that though his form and thought are not Greek, yet his methods are) often falls, I fear, under the former condemnation. On the other hand, a man of so sincere and high a soul as Dante did not disdain to employ petty artifices as means to a greater end, for even in the ecstasy of the *Paradiso* he resorts to device after device to convey the idea of the ever increasing light as he rises from heaven to heaven, until, when we think his resources exhausted, he startles us with a still more glorious conception of the divine radiance. But the light is only a means to express the joy of dwelling ever closer and closer to God. Thus does Aeschylus use dramatic artifices, as the means natural to him for the expression of his thought; but when he feels that they hamper his freedom, he is the first to cast them aside.

AN EXAMINATION OF THE THEORIES REGARDING THE NATURE AND ORIGIN OF INDO–EUROPEAN INFLECTION

By Hanns Oertel and Edward P. Morris

I. INTRODUCTION (§ 1–2).

1. The necessity of forming a definite theory concerning the origin of Indo-European inflectional endings, because the method of dealing with concrete problems of the development of cases, modes, and tenses in the historical Indo-European languages is essentially influenced by such a theory (§ 1).

2. Three preliminary remarks and limitations (§ 2) :

   A. The discussion refers to the Indo-European languages only.

   B. In assigning a language to the 'adaptation type' we do not intend to exclude the possibility that some agglutinative forms may be found in it.

   C. The importance of distinguishing between *formal* and *semantic* agglutination.

   Summary of the four positions which may be taken regarding agglutination.

II. AGGLUTINATION AND ADAPTATION (§ 3–18).

A. FORMAL AGGLUTINATION (§ 3–5).

   1. Ludwig's belief in formal agglutination (§ 3).
   2. Jespersen's 'sound-*continua*' (§ 4).
   3. Discussion of the relative probability of the two theories (Bopp's and Jespersen's). The advisability of an agnostic attitude regarding primitive Indo-European word-formation (§ 5).

B. SEMANTIC AGGLUTINATION (§ 6–18).

   1. The alternative theory (adaptation) proposed by Ludwig. His view regarding undoubted cases of semantic agglutination in the historical periods of the Indo-European languages (§ 6).

2. The argument against semantic agglutination and in favor of adaptation as the typical manner in which the Indo-European inflectional system originated (§ 7–18).

(*a*) The weakness of Ludwig's Vedic support for his theory (§ 7).

(*b*) The weakness of the case for semantic agglutination (§ 8–9).

(α) The number of instances where inflectional suffixes can actually be identified with formerly independent words is very small (§ 8).

(β) Bopp's theory of semantic agglutination was not the natural and simple consequence of his analysis of Indo-European inflectional forms, but owed its origin to extraneous influences and *a priori* considerations (§ 9).

(*c*) The certain cases illustrative of semantic agglutination can be paralleled by equally well established instances of adaptation (§ 10).

(*d*) The chief argument in favor of adaptation and against semantic agglutination for the Indo-European languages is the qualitative difference between their inflectional structure and that of the 'agglutinative' type of languages, e. g. the Ural-Altaic (§ 11). Detailed examination of this structural difference between 'inflecting' and 'agglutinative' languages (§ 12–13):

(α) The regular and systematic character of the agglutinative languages. The stability of their inflectional systems. Their tenacity in maintaining their case-system (§ 12).

(β) The irregular and complex character of Indo-European structure. The mobility of the Indo-European inflectional suffixes (§ 13).

(*e*) The lack of system of Indo-European inflectional structure in the light of the theory of adaptation (§ 14–17):

III. **THE EFFECT OF THE FOREGOING ON THE IDEA OF A 'GRUNDBEGRIFF'** (§ 19–31).

§ 1. There is at present, and there has been for some time, a dis-
inclination amounting occasionally to a positive aversion to discuss
problems of origins, which by their very nature must be metaphysical or
rather 'metagrammatical.' Undoubtedly Delbrück is right in closing
the fourth edition of his *Einleitung* (1904) with the words : ' For the
near future glottogonic hypotheses will probably remain in the back-
ground, while the historical and psychological investigation of the actual
data will, we hope, progress further and further.'

And yet the very fact that these problems crop out again and again,
and that even those who are honest in their renunciation are often
forced tacitly to assume some kind of hypothesis shows that these
discussions are not due simply to a love of speculation — that Greek
vice of which Crassus (*Cic. de Orat.* 2, 4, 18) said, *omnium ineptiarum
haud sciam an nulla sit maior quam . . . de rebus aut difficillimis aut
non necessariis argutissime disputare.* Why is it that we cannot refrain
entirely from such speculation and subscribe to Johannes Schmidt's[1]

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[1] *Kuhn's Zt.* XXIV (1879), p. 320 f. ' Die aufgabe der indogermanischen
sprachwissenschaft ist, nachzuweisen, welches die formen der ursprache waren, und
auf welchen wegen daraus die der einzelsprachen entstanden sind. Den begrifflichen

confession of honorable ignorance? There are two reasons, both clearly felt and expressed twenty years ago by Delbrück.[1] (1) The attempts to analyze grammatical forms are not simply airy speculations, they do not rest wholly upon assumptions and imaginary constructions of single scholars, but are ultimately based upon a considerable mass of definite linguistic facts. (2) The carrying on of work upon one whole class of problems in syntax — the history and use of modes, tenses, and cases — forces the investigator, unless he is content with mere description, to adopt some hypothesis in regard to the early history of inflection;[2] without such a hypothesis an interpretation of the facts is impossible.

It is for this reason that we propose in the following paper to call the attention of classical philologists to the gradual shift of opinion among comparative philologists in regard to the long-held theory of agglutination as applied to the Indo-European family of languages, and to suggest or discuss some of the effects which this changed attitude necessarily produces upon our views of semantics and of syntax in particular.

§ 2. Before entering upon the discussion it seems advisable to make three points clear at the outset. (1) The discussion of the theories of agglutination and adaptation in the following pages refers to the Indo-European group of languages only. We do not for a moment hold that what seems to us true with regard to them applies to other families also. In fact, we are strongly convinced that the different types of languages show essential differences in their development (see below, §§ 11 to 18, pp. 81–98), and we would therefore discourage the practice of interpreting the evolution of one clearly defined and characterized

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werth der an die sogenannten wurzeln gefügten formativen elemente zu erklären sind wir in den allermeisten fällen . . . unfähig. . . . Auf diesem gebiete schreitet, wie es einer gesunden wissenschaft ziemt, die erkenntniss des nichtwissens von jahr zu jahr fort.'

[1] Delbrück, *Einleitung in das Sprachstudium* (1880), p. 100. 'Ich glaube aber doch nicht dass diese [Schmidt's] Anschauungsweise allgemein werden wird. Die Versuche, die Sprachformen zu zerlegen, beruhen schliesslich nicht auf willkührlichen Entschlüssen und Einfällen der Gelehrten, sondern haben gewisse sprachliche Thatsachen zur Grundlage . . . und werden also vermuthlich auch in Zukunft wiederholt werden.'

[2] Delbrück, *Einleitung in das Sprachstudium* (1880), p. 91, ' . . . bei öfter wiederholter Erwägung der ganzen Frage [viz. the question of the origin of case endings], auf die ich bei syntaktischen Arbeiten immer wieder geführt worden bin . . .'

family of languages by borrowing illustrations of development from languages in no way related to it. Such 'analogies' are to be used with great caution; they often cloud, rather than clear, the issue. (2) When we speak of either 'agglutination' or 'adaptation,' we do not mean these terms to be taken in an exclusive sense, as if in a language of the agglutinative type no cases of adaptation could possibly occur, or, *vice versa*, no cases of agglutination in a language of the adaptive type. The discovery, therefore, of such exceptional cases will not disturb our classification of a language, since this rests upon the general characteristics of its structure (cf. pp. 75, 92, 98). (3) A distinction should be made — and we have endeavored strictly to maintain it throughout this paper — between formal and semantic agglutination. As applied to Bopp's theory, the term agglutination[1] was first and primarily a morphological term. Bopp's analysis of words into stems and affixes was first of all an attempt to explain the Indo-European inflectional system on the formal side. But it also, almost of necessity, both sought for and found support from the semantic side and in turn influenced the semantic analysis of Indo-European words. As introduced to classical philologists by Curtius, it was in reality a semantic as well as a formal hypothesis. The most familiar example is the formal analysis of the Greek optative φέροιμι into φέρ-ο-ι-μι and its semantic interpretation by the identification of the -ι- with the root of the verb 'to go' (ἰέναι). After this step had been taken, it seemed not unnatural to derive the historical meaning of the mode, as well as its form, by a succession of modifications of the compound: 'I go to bear' > 'I am about to bear' > 'I would bear,' and so on.

This confusion of form and meaning we hold to be harmful and we desire, if possible, a greater precision in the treatment of meanings by separating the origin and history of meanings from the origin and history of forms.

And in the particular question of agglutination it appears to us to be absolutely necessary to distinguish between (*a*) supposed agglutination of formal elements (morphological composition), and (*b*) the very different hypothesis of semantic agglutination, which holds that the

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[1] According to Delbrück, *Einleitung in das Sprachstudium* (1880), p. 15, note 1, it was Lassen who coined this term.

original total meanings of whole words and inflectional forms were built up into complexity by the synthesis or agglutination of simpler elements of meaning, as when the total force of the form φέροιμι was thought to be the result of a synthesis of the verbal stem φερ-ο- meaning 'to carry' with the stem -ι- of the verb 'to go.'

From this discussion it will appear that four attitudes are possible with regard to agglutination :

(1) We may with Bopp and his successors believe in both formal and semantic agglutination.

(2) We may reject both formal and semantic agglutination ; this was the position of Friedrich v. Schlegel[1] and those who followed him, like R. Westphal. Purged of its biological taint, this theory, as we shall see below (§ 4, p. 71), is again advanced by Jespersen.

(3) We may hold to the theory of formal agglutination, while rejecting that of semantic agglutination. This the next section will show to have been Ludwig's belief.

(4) We may take a position of complete agnosticism as to the formal aspect of the question, while denying semantic agglutination.

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§ 3. Inasmuch as Ludwig is commonly regarded as the champion of the theory of adaptation it is important to note at the outset that Ludwig uses the terms 'agglutination' and 'adaptation' in a semantic sense only. These two terms with him do not refer at all to the formation of words, but to the development of meanings. He does not hold that there was no such process as the formal building up of stems (or words) by the addition of suffixes to roots. 'We distinguish,'[2] he wrote in 1867, 'the period of roots ; the period when one or more roots were reduced to a pronominal meaning and were used as pronouns ; *the union of these with roots, i. e. the creation of words.*[3] Further there ensued loss of the pronominal and deictic force in these elements, which came to be the carrier of the idea of the *agens, actum*, etc., which had formerly rested in the root alone. Thus the pronoun changed into

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[1] Cf. on Schlegel's 'Evolutionstheorie,' Delbrück, *Einleitung in das Sprachstudium* (1880), p. 62 (= p. 75 of the third edition; in the fourth edition, p. 129, it is barely touched).

[2] *Sitzungsber. Wien. Acad.* LV (1867), p. 134.

[3] Cf. also the quotation on p. 104.

a word-formative suffix. Traces of this period are preserved in compounds. . . . The last use to which these elements are put is that of inflection.' From this and the table appended it will readily be seen

<div align="center"><small>DEVELOPMENT OF THE INDO-EUROPEAN INFLECTIONAL SYSTEM
ACCORDING TO LUDWIG</small></div>

Prehistoric Periods	1st Period: Independent 'roots.' Differentiation into independent nominal[1] and pronominal 'roots' ('full words'[2] and 'demonstrative roots').	Root Period	Pregrammatical Periods
	2d Period: Union of nominal[1] with pronominal roots, the latter retaining their deictic meaning.	Period of word creation	
	3d Period: The pronominal roots, as members of a word, lose their deictic force, become real suffixes and acquire new meanings from the nominal[1] roots to which they are joined.	Period of word-formative suffixes	
Historic Period	4th Period: Some of these word-formative suffixes are utilized to express inflectional categories.	Inflectional Period	Grammatical Period

that Ludwig set apart a distinct period (the second) for the building up of words from originally independent elements, and that the suffixes of the third period were originally independent in form.

§ 4. For these earliest periods he agrees therefore, on the formal side, with Bopp's synthetic theory and differs from that of Jespersen, who denies the original independence of those formative elements

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[1] *nominal* includes here *verbal*.

[2] Misteli, *Charakteristik der hauptsächlichsten Typen des Sprachbaues* (1893), p. 516, suggests the term 'qualitative' roots: 'Man möchte jene Classe, welche die Welt der Dinge benennt, qualitative Wurzeln nennen; sie bezeichnen Qualitäten, oder die Dinge nach ihren Qualitäten; die andere Classe mag demonstrativ heissen, weil sie die Dinge nicht qualitativ benennt, sondern nur vom Standpunkt des Redenden aus auf sie hinweisst.'

which later appear as suffixes. 'If,' Jespersen writes,[1] 'we accepted "synthesis" as the designation of the earliest stage, we should be guilty of inconsistency. For as *synthesis* means composition, putting together, it presupposes that the elements "put together" had at first an independent existence; and this we deny. Therefore, whoever does not share the usual opinion that all flectional forms have originated through independent words gradually coalescing, but sees that we have sometimes to deal with the reverse process of inseparable parts of words gradually gaining independence, will have to look out for a better or less ambiguous word than *synthesis* for the conditions of primitive speech. What in the later stages of language is analyzed or dissolved, in the earlier stages was unanalysable or indissoluble; "entangled" or "complicated" would therefore be better renderings of our impression of the first state of things.' And while he does not go so far as to deny all synthesis in early times [2] he yet restricts it to a very small number of cases, and his discussion of the origin of language is entirely based

DEVELOPMENT OF THE INDO-EUROPEAN GRAMMATICAL SYSTEM
ACCORDING TO JESPERSEN

1st Period: Sound-*continua*, fairly long and of very rich semantic contents evenly distributed over the whole. 'Sentence-words.'	Complex Period
2d Period: The semantic contents are gradually analyzed and definite semantic elements attached to definite formal elements. Origin, by separation, of formative elements which acquire a meaning of their own.	Analytical Period

upon the theory of early 'sentence words,' very complicated in point of meaning and fairly long in point of sound.[3]

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[1] *Progress in Language*, 1894, p. 347, § 269.

[2] *Progress in Language*, p. 66, § 56: 'Now, of course, it cannot be denied that similar processes [of adding originally independent words, i. e. of formal agglutination] may have been going on at any time and that some flectional forms of old Arian may have arisen in this way. But when the inference is that they are *all* to be explained in this manner, and that here we have the key to flection in general, great exception may be taken.'

[3] *Progress in Language*, p. 347, § 270: 'The direction of movement is towards flectionless languages (such as Chinese, or to a certain extent Modern English) with

§ 5. It is impossible to decide the case between Bopp's and Jespersen's hypotheses upon the basis of either historical or comparative grammar, and Brugmann therefore very properly declines to enter upon the question of the origin of formatives (suffixes and root-determinatives) : 'These[1] terms [suffix, prefix, etc.] all presuppose a certain[2] original independence, such as the members of a compound like modern High German *gottes-gabe.* While this supposition is true, in general, for prefixes, it holds good for a small number of suffixes only ; and the suffixes which can be shown to have at one time existed as separate words (such as *-heit* in *schön-heit,* originally 'beautiful condition'; in Middle and Old High German *heit* still occurs as an independent word) are almost all not common Indo-European property, but occur in single languages only. In the case of hardly any primitive Indo-European suffix do we have the right to interpret the boundary lines which our grammatical analysis establishes as the original joints or boundaries between two independent words ; for the original meaning of these suffixes cannot be etymologically determined and, for many reasons, it would be improper to explain all cases after the analogy of *schön-heit.*

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freely combinable elements; the starting point was flectional languages (such as Latin or Greek); at a still earlier stage we must suppose a language in which a verbal form might indicate not only six things like *cantavisset* [348], but a still larger number, in which verbs were perhaps modified according to the gender (or sex) of the subject, as they are in Semitic languages, or according to the object, as they are in some American Indian languages. But that amounts to the same thing as saying that the borderline between word and sentence was not so clearly defined as in more recent times; *cantavisset* is really nothing but a sentence word, and the same holds true to a still greater extent of the sound-conglomerations of Indian languages. . . . It will be noticed that in speaking of "sentence words" as the original units of language I do not use that expression in exactly the same sense as certain linguistic writers, who exemplify their notion of primitive sentence words by such modern instances as "Fire !" or "Thief!" In my opinion primitive linguistic units must have been much more complicated in point of meaning, as well as much longer in point of sound.' Cf. also p. 363, § 284, commencing: 'Again, we saw above that the further back we went, the more the sentence was one indissoluble whole, in which those elements which we are accustomed to think of as single words were not yet separated,' etc.

[1] Brugmann, *Kurze Vergleich. Gramm.* II (1903), p. 284, § 365.

[2] We cannot see why 'independence' should be thus modified and restricted. If these elements were independent at all, they must have been wholly independent at some period.

There are hardly any primitive Indo-European suffixes the etymology of which is not hopelessly obscure.' And :[1] 'How these determinatives arose is hidden from our knowledge. It is possible that like the so-called suffixes some may originally have been independent words. In general the line of demarcation between suffixes and determinatives is not always sharply drawn.'[2] This attitude is shared by Delbrück,[3] who views the terms 'root' and 'suffix' simply as convenient grammatical abstractions, but is indifferent to the question of their origin; 'the question how the *ie̅* of the optative or the *s* of the aorist might possibly be explained, is left open as unanswerable. . . . We occupy at present a position of resigned agnosticism regarding all problems of origin.' If the problem admits of any solution at all this can come only from general psychological considerations. Such is Wundt's[4] argument against Bopp's synthetic hypothesis, based upon the relation of word to sentence : 'If the word precedes the sentence, if the latter is originally a synthesis of words, as we now combine words into sentences, then we can hardly escape the conclusion that "roots" of some kind . . . were the primitive material of speech. For the analysis of words does not permit us to doubt that, in general, words are complicated formations. . . . If therefore words were originally isolated which only later were combined with others for the formation of sentences, then we can hardly help inferring that the elements of a word also, at one time, led an isolated existence. If, on the other hand, the sentence precedes the word, and the latter arose from the former by a separation of the sentence-whole into smaller units, then the elements of a word also are not to be regarded as originally isolated.'[5] Perhaps we might also base some

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[1] Brugmann, *ibid*. II, p. 297, § 367. Cf. now also Thurneysen, *Die Etymologie*, 1904, p. 11 ff.

[2] Cf. also *ibid*., p. 418, § 529: 'weil die Herkunft der Kasusformantien unklar ist.'

[3] *Einleitung in das Studium der Indogermanischen Sprachen*[4] (1904), p. 137.

[4] *Völkerpsychologie*. *Die Sprache*, I (1900), p. 557. Compare with this Wunderlich, *Der deutsche Satzbau*[2] (1901), I, pp. xiv and xix.

[5] See on this also Delbrück, *Grundfragen der Sprachforschung* (1901), p. 119. What Wundt (*Sprachgeschichte und Sprachpsychologie*, 1901, p. 87) and Sütterlin (*Das Wesen der sprachlichen Gebilde*, 1902, p. 59) argue against Delbrück does not touch the point here in question.

inferences upon the speech of children, if only we had a sufficient
number of words which are not simply reproductions, more or less
perfect, of the speech of adults.[1]   In the case observed by Miss E. H.
Watson and discussed by Horatio Hale[2] the children used certain
peculiar words, such as *ni-si-boo-a* for 'carriage,' which in themselves
were polysyllabic and, by repetition of syllables, were even further
lengthened.

It thus appears that the problem of Indo-European morphological
word architecture is, if anything, an even greater riddle than it was
twenty years ago, when Delbrück[3] answered the question 'Hat sich
denn nun die Agglutinationstheorie im Einzelnen bewährt?' with far-
sighted scepticism: 'Ich möchte kaum glauben, dass der geduldige
Leser, welcher mir durch die ganze vorstehende Ausführung [pp. 73–
101] gefolgt ist, mit einem zuversichtlichen Ja antworten wird.   Denn
im besten Fall hat sich uns für die Einzelanalysen eine gewisse Wahr-
scheinlichkeit, nicht selten das kahle *non liquet* ergeben.'

§ 6.   While, as we have seen above (§ 3), there is no essential differ-
ence between Ludwig and Bopp regarding formal agglutination, a wide
gulf separates the semantic interpretation of Indo-European inflectional
forms of the former from that of the latter.   Ludwig[4] does not use the
term agglutination to describe the building up of stems by formal addi-
tions, such as Sk. *rāj-an-*, 'king,' from *rāj-*, 'king,' and *bhū-mi-*, 'earth,'
from *bhū-*, 'earth'; he confines the term to a special process, namely,
the merging into one semantic whole of two originally independent
words, each one contributing its primary meaning to the resultant whole.
Now while he admits 'that even in the earliest periods the beginnings

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[1] Wundt, *Die Sprache*, I (1900), p. 296 with note.

[2] *Proceedings of the Amer. Assoc. for the Advancement of Science*, XXXV (1886),
p. 287.   Cf. Meumann, *Die Sprache des Kindes* (1903), p. 31.

[3] *Einleitung in das Sprachstudium* (1880), p. 101.

[4] His theory is set forth in four papers: *Ovyvinuti řeckých vět podmínečnych s
předběžnou úvahou o historii skladby* in *Krok*, 2, Prague, 1865 [this article we have
not seen]; *Die Entstehung der a-Declination. Ein Beitrag zur Geschichte der
Wortbildung im Indo-Germanischen*, in the *Sitzungsberichte (phil.-hist. Classe)* of
the Vienna Academy, 1867, LV, p. 131; *Der Infinitiv im Veda mit einer Systematik
des litauischen und slavischen Verbs*, Prague, 1871; and *Agglutination oder Adaption.
Eine sprachwissenschaftliche Streitfrage*, Prague, 1873.

[of such agglutinative word-formation] may undoubtedly be detected,'[1] such cases are to Ludwig exceptional, — anticipations, as it were, of a manner of word-formation which really belongs to a later period, and for which the period of adaptation (his third period) had to prepare the way. For as the Indo-European languages gradually learnt to analyze their polysyllabic words (created in his second period) and to localize part of the meaning of the whole word in its termination, charging with definite grammatical significations the suffixes which by that time had faded and become entirely colorless, there arose a contrast between that part of the semantic whole which appeared to rest in the 'root' and that part which was carried by the suffix. After the speaker had once distributed the contents of a word like *pulchritudo* or *daubiþa* so as to connect the idea of quality ('condition of being') and nominal force with *-tudo* and *-þa*, he might well go a step farther and in new formations select an even clearer and more definite expression for this particular idea, e. g. the independent noun 'quality' (Middle High German *heit*) and thus proceed from *daubiþa* to *taubheit*.[2] So that, for the Indo-European languages, the process of semantic agglutination would appear to be rather a new departure. The large majority of suffixes (including mode, tense, and case formatives) did not originally possess their later meanings at all, but received them after they had united with the radical elements into words (third period). This gradual acquisition of a definite new semantic (functional) content by suffixes which originally had no trace of their later meaning Ludwig calls adaptation. And to us the really essential feature of his *Adaptationstheorie* is the belief that the force which almost all suffixes have in the historical Indo-European languages is of secondary character. 'Whether we derive *-mai, -sai, -tai* from *-mami, -sasi, -tati*, or from *-maki, -saki, -taki*, or from any-

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[1] *Ibid.*, p. 21. For the reduction of second members of compounds to suffixes see Brugmann, *Grundriss*, II, pp. 3, 7, 9 ff.; *Kurze Vergl. Gramm.* II, p. 289; Wilmanns, *Deut. Grammat.* II, p. 6, § 4; Richter, *Indog. Forsch.* IX (1898), p. 223. The same process can be observed in the so-called agglutinative languages; cf. Grzegorzewski, *Sitzungsberichte* (*phil.-hist. Classe*) of the Vienna Academy, 1903, CXLVI, p. 57 ("volle Verschmelzung des Stammes mit dem zweiten Gliede, das infolge dessen zur vollkommenen Endung herabsinkt"; so *karatti* for *karat-ätti*, *vikoräňcti* for *vikoräňč-etti*).

[2] *Ibid.*, p. 21–22.

thing else you please, is of very small importance compared with the fundamental question : are *-mai, -sai, -tai* [originally] identical with their [later] meaning or are they mere vehicles of it?'[1]

§ 7. In attempting to reach an adequate judgment of this theory it is necessary to dissociate it from certain phonological idiosyncrasies[2] which are the result of Ludwig's desire to reduce to the smallest possible number[3] the mass of historical suffixes, and to examine it quite apart from the Vedic material by which he sought to support it. For the close union of his linguistic speculations with his Vedic interpretations has been disastrous to both. In Vedic grammar, on the one hand, a notable observation regarding the curtailment of inflectional forms remained unnoticed because of its connection with an unacceptable linguistic hypothesis,[4] until Roth, independently, presented the matter

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[1] *Agglut. od. Adapt.*, p. 41 : 'sind *mai*, etc., identisch mit ihrer Bedeutung oder bloss Träger derselben?'

[2] Delbrück, *Einleitung in das Studium der Indogermanischen Sprachen*, 4th ed., 1904, p. 132 : 'Um diese Theorien im Einzelnen durchzuführen, nimmt Ludwig eine Anzahl von Lautgesetzen an, die von dem, was anderen Gelehrten als feststehend gilt, erheblich abweichen.' Cf. Benfey, in his review of Ludwig's *Infinitiv im Veda* in *The North British Review*, LIII (1871), p. 535: ' . . . while he inveighs against the bold hypotheses of many critics with regard to phonetic changes, he himself is far from being moderate in the use of them.'

[3] *Agglut. od. Adapt.*, p. 25 : 'Hieraus nun ergibt sich weiter nicht nur das recht sondern die notwendigkeit, die suffixe auf eine möglichst geringe zal von ausgangs-formen zu reducieren.' Though he immediately adds : 'eine reduction die ihre grenze nur in den von mir stets gewissenhaft berücksichtigten lautgesetzen finden darf.'

[4] Pischel und Geldner, *Vedische Studien*, I, p. xviii f.: 'Er selbst [Ludwig] nimmt Formen an, die, wie er behauptet, die philologische Interpretation fordert, und die Erfahrung hat ihm darin zum Teil recht gegeben. So hat Ludwig bereits lange vor Roth die Wortkürzung erkannt, die für die Erklärung vieler Stellen von höchster Wichtigkeit ist. Statt sie aber rein mechanisch zu erklären, hat er sie für seine lingu-istischen Theorien zu verwerten gesucht und deshalb keine Beachtung gefunden.' Cf. also Pischel's discussion, *ibid.* II, p. 237. And on the whole subject of the mechanical abbreviation of inflectional forms, Wackernagel, *Altind. Grammat.* I, p. xvii with note 3. To the references given there might be added, Hopkins, *J A O S*. XXIII, p. 111; Richter, *I. F.* IX, 29, and Steglich, *Über die Erstarrung von Flexions- und Bildungssilben bei copulativen Verbindungen* in *Zt. f. deutsche Wort-forschung*, III (1902), pp. 1–52.

in a different form[1] in 1886. On the other hand, Ludwig's insistence
that certain peculiarities of Vedic inflection were necessarily survivals of
a pre-grammatical period and could thus furnish absolute proof for his
linguistic theory made its correctness wholly dependent upon the
explanation of these Vedic forms and thus, unfortunately, but neces-
sarily, narrowed the discussion down to questions of Vedic exegesis and
Sanskrit grammar.[2] And yet, if it is true, as Lessing once said,[3] that a
mathematical truth might be discovered by a syllogistic error and yet
remain no less useful a truth, though the error be subsequently dis-
covered and disclosed, it does not follow that because the Vedic support
has been taken from under Ludwig's hypothesis, the latter must, of
necessity, wholly collapse. While some parts may fall, some essential
features may be found in harmony rather than in disagreement with
more recent currents and theories of comparative grammar. We shall
discard, therefore, the concrete Vedic proof used by Ludwig to support
his theory and in the next four sections shall review the arguments
which have seemed to us important in determining our attitude toward
the hypothesis of adaptation.

§ 8. Since, in reality, the hypothesis of semantic agglutination is by
no means self-evident, it needs the support of positive proof just as
much as the contrary hypothesis. Such positive proof may be derived
from two sources. In the first place, the etymological analysis of
primitive Indo-European formatives might show their original inde-
pendence. This road, however, as we have seen above, is admitted to
be impassable, and it is noteworthy that the number of suffixes which
were believed to be etymologically clear has steadily decreased, until
now even that stronghold of Bopp's agglutinative theory, the pronominal
origin of the personal endings of the verb, is sorely beset.[4] On the

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[1] *Über gewisse Kürzungen des Wortendes im Veda*, in *Berichte des VII. inter-
nationalen Orientalisten Congresses. Arische Section.* Wien, 1888, pp. 1–10.

[2] So in Benfey's and Delbrück's reviews of the *Infinitiv im Veda*. Cf. *The North
British Review*, LIII (1871), p. 532, and *Kuhn's Zt.* XX (1872), p. 216.

[3] *Ed. Lachmann*, X (1839), p. 38.

[4] Against the connection of the inflectional terminations of the finite verb with
personal pronouns see Sayce, *Academy*, Sept. 16, 1881, XXII, p. 208; Techmer's
Internationale Zeitschrift, I (1886), pp. 222–5; *Introduction to the Science of
Language*[3], II (1890), p. 151; and Hirt, *Indog. Forsch.* XVII (1904), pp. 64–83.

other hand, we might support Bopp's theory by pointing out a fairly large number of cases in the more recent periods of Indo-European[1] languages. But Jespersen[2] has shown that 'the number of actual forms proved beyond a doubt to have originated through agglutination is very small; the three or four instances named above [viz. the Scandinavian passive voice, the Romance future tense, and the Scandinavian suffixed article] are everywhere appealed to, but are there so many more than these? And are they numerous enough to justify so general an assertion?' If the number of well-established cases of semantic agglutination in the Indo-European languages is so small, a number quite inconsiderable in comparison with the total mass of inflectional forms, we must seriously question whether it is safe or even permissible to base a general theory[3] on evidence that may very well be quite exceptional. Nor is it

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The first to attack Bopp's theory seriously seems to have been R. Westphal, *Philosophisch-historische Grammatik der deutschen Sprache*, 1869 (see the review in the *Litterarische Centralblatt*, 1869, No. 9, col. 236 ff.), and *Vergleichende Grammatik d. indogerm. Sprachen*, I (1873), preface, pp. xxiii–xxxviii. He tried to elaborate a theory according to which the independent personal pronouns are really detached verbal terminations; cf. especially p. xxviii, 'But is the fact that Bopp's explanation of the genesis of inflectional endings is the only one possible really so firmly established? At first glance it seems probable that the verbal terminations arose by composition of the root with pronominal stems. But this process, though seemingly probable, may have to be reversed to reach the truth. We must remember that for a long time ocular proof of the sun's motion around the earth was accepted until the progress of science reversed it. And so in our case, everything seems to point to the fact that the historical relation which has heretofore been held to exist between pronominal stems and verbal inflection should really be reversed: verbal inflection antedates the pronominal stems.'

[1] It is necessary to confine ourselves to *Indo-European* languages. For Ludwig's Adaptationstheorie is not a general theory applicable to all language-growth, but expressly designed to apply to Indo-European inflection, and, as will be shown (pp. 85, 96), Ludwig considered it one of the strongest arguments in favor of his theory that it clearly established a contrast between inflecting and agglutinative languages. No support can therefore be derived from such a typically different linguistic family as the Ural-Altaic (Delbrück, *Einleitung in d. Studium d. Indogerm. Sprachen*[4] (1904), p. 134).

[2] *Progress in Language*, p. 66, § 56 (cf. p. 63, § 54).

[3] The necessity of evidence from the historical languages to take the place of the now discarded etymological evidence was expressed by Delbrück in 1880 (*Einleitung*, p. 72): 'Die Beobachtung, dass in historischer Zeit Sprachformen durch Zusammen-

to be forgotten that the force of late examples of semantic agglutination is weakened by the considerations detailed on p. 75.

§ 9. As a matter of fact, however, Bopp did not evolve his hypothesis of semantic agglutination from the facts presented in the Indo-European languages, nor is that hypothesis the result which would naturally follow from Bopp's analysis of Indo-European inflections. A glance at the admirable sketch of his work in Delbrück's *Einleitung* will show that Bopp read his theory into the facts rather than drew it from them. The considerations which led him to give up Schlegel's 'Evolutions-theorie,' which he had at first held,[1] and gradually to extend further and further the application of the agglutinative theory[2] to Indo-European forms, until he finally made it the universal explanation of inflections, were three in number : (*a*) By the medieval logical analysis of the judgment into subject, copula, and predicate he was led to identify[3] the -*s*- of the aorists and futures with the stem of the verb 'to be.' (*b*) The analysis of an inflectional form into root and suffix he took from Hebrew grammar.[4] (*c*) The assumption that all roots were monosyllabic was a tenet of traditional grammar and had been strengthened by the doctrine of the native Hindoo grammarians.[5] Now if the slightness of the positive evidence for agglutination is further weakened by the fact that the theory was so largely shaped by these extraneous influences, it seems to us hardly possible to save even 'the

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setzung entstehen, muss zu Gunsten der gleichen Annahme für die sog. vorhistorische Zeit schwer in's Gewicht fallen.'

[1] Delbrück, *Einleitung*[4] (1904), p. 58, p. 61 (line 23, 'immer ausschliesslicher,' etc.), and p. 63, last section.

[2] Delbrück, *Einleitung*[4] (1904), p. 27, last section.

[3] *Ibid.*, pp. 23–24.

[4] *Ibid.*, pp. 26–28, especially the last section of p. 28, and compare the reference to Arabic analogy in the quotation from Bopp, p. 60.

[5] *Ibid.*, pp. 24, 59, 135. This view is substantially the same as that offered in the first edition of the *Einleitung* (1880), p. 3: 'Die Bopp'schen Theorien über die Genesis der Sprachformen sind nicht etwa (wie man annehmen könnte) das reine Ergebniss seiner grammatischen Analyse, sondern sie gehen zu einem sehr wesentlichen Theile auf ältere Anschauungen und Vorurtheile zurück' (cf. also p. 73), and p. 97: 'Wie oben gezeigt worden ist, wurde Bopp hauptsächlich durch einen scholasti-schen Irrthum betreffend die drei Redetheile zu seiner Hypothese geführt, dass in dem *s*-Aorist und dem Futurum die Wurzel *as* stecke.'

general probability of Bopp's theory of inflection,' so far as the bulk of Indo-European forms is concerned.[1]

§ 10. A few isolated cases cannot be considered sufficient to prove so important and far-reaching a theory. Evidence of a similar character and of equal weight might easily be given from modern Indo-European languages to show that endings have originated in a different manner and have been charged with meaning by a process of adaptation. Such cases have been collected by Jespersen,[2] e. g., *-en* in English *oxen*, *-er* in German *rinder*, which are in reality simple *-n-* and *-s-* stems, whose final syllable has been turned into a plural suffix.[3] So far, therefore, as the scanty testimony of modern languages goes, the two hypotheses seem to be equally well supported.

The truth is, however, that neither theory can be said to make out a strong case on such concrete evidence ; our choice must be guided by general considerations.

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[1] It would of course be rash to deny the possibility that some few early forms may have an agglutinative origin. But this was admitted even by Ludwig (*Agglut. od. Adapt.*, p. 21; above, p. 75). Delbrück, in his latest work (*Einleitung*[4], p. 134), though he still feels inclined to hold to Bopp's theory (' so dass ich mich doch wieder auf die Boppsche Anschauung zurückgewiesen sehe '), yet limits its application (' dass die Zusammensetzung dabei [bei der Bildung der Flexionsformen] mindestens auch eine Rolle gespielt habe '). Cf. Misteli, *Charakteristik der hauptsächlichsten Typen des Sprachbaues* (1893), p. 501: ' Und sollte es auch gelingen, die eine oder andere Endung auf ein selbständiges Wort zurückzubringen, z. B. die . . . Medial-endungen auf den Stamm des Reflexivs, was nach Laut und Begriff ganz gerechtfertigt wäre, oder nach heute beliebter Annahme das imperativische *-tōd* auf den Ablativ *tōd* von *to-*, etwa im Sinne von *tāvat*, τέως, *dum*, die sich im Sskrt. Griech. Lat. so oft mit Imperativen verbinden: die grosse Masse wird auf diesem Wege keine Erklärung finden.' Thurneysen, *Die Etymologie*, 1904, p. 11–12.

[2] *Progress in Language*, pp. 67–69, § 57.

[3] These cases furnish a good parallel to the development of the feminine singular or rather ' collective ' (Michels, *Germania*, XXXVI, 1891, p. 132) ending *-ā* into the ending of neuter plural (Brugmann, *Griech. Gramm.*[3], p. 235; *Kurze Vergl. Gramm.* II, § 435, p. 355), a development closely similar to that of the *plurales fracti* in the South Semitic dialects (Barth, *Die Nominalbildung in den semitischen Sprachen*, 1894, p. 417 ff.). — By exactly the opposite process part of the personal ending of the third person sg. Lithuanian *ei-ti* ' he goes ' is annexed to the verbal stem in the first person *eit-ù* ' I go ' (Brugmann, *Kurze Vergl. Gramm.* II, § 633, Anm. **2**, p. 490, and § 685, Anm., p. 521).

§ 11. The most important of these general considerations is drawn from an examination of the recognized 'agglutinative' languages (the Ural-Altaic branch, for instance) and from the contrast between their structural peculiarities and the architecture of the 'inflecting' languages. Such an examination and contrast will show, we believe, that the former type is characterized by regularity and symmetry of system, while the latter is essentially irregular, unsystematic, and complicated in character. And this difference, we hope to show, is due not to any arrested development or the like, but to a fundamental difference in the plan on which the two types respectively are built. In fact, we hold that the peculiar inflectional and syntactical phenomena of the languages of the Indo-European type are an insurmountable obstacle in the way of applying a theory of semantic agglutination to them.

§ 12. In order to make the contrast plain we give examples, first from the Turkish.[1] In the declension of nouns there are special suffixes for number and for case, each retaining its distinctive form and meaning. Thus nom. sing. *er* 'man,' plur. *er-ler* 'men'; gen. sing. *er-iñ*, plur. *er-ler-iñ*; loc. sing. *er-dé*, plur. *er-ler-dé*, etc. In verbs the clearness of composition is equally marked; we give illustrations of a paradigm (all forms are infinitives with suffix *-mek*; the other suffixes are *-me-* for the negative, *-e-me-* for the *impossibilis*, *-il-* for the passive, *-dir-* for the causative, *-ish-* for the reciprocal, *-in-* for the reflexive) :

Simple Active.		Affirmat.:	*sew-mek* 'to love.'
		Negative:	*sew-me-mek* 'not to love.'
		Impossib.:	*sew-e-me-mek* 'not to be able to love.'
Simple Passive.		Affirmat.:	*sew-il-mek* 'to be loved.'
		Negative:	*sew-il-me-mek* 'not to be loved.'
		Impossib.:	*sew-il-e-me-mek* 'not capable of being loved.'
Causatives.	A.	Affirmat.:	*sew-dir-mek* 'to cause to love.'
		Negative:	*sew-dir-me-mek* 'not to cause to love.'
		Impossib.:	*sew-dir-e-me-mek* 'not to be able to cause to love.'
	B.	Affirmat.:	*sew-dir-il-mek* 'to be made to love.'
		Negative:	*sew-dir-il-me-mek* 'not to be made to love.'
		Impossib.:	*sew-dir-il-e-me-mek* 'not capable of being made to love.'

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[1] The examples are from Wahrmund's *Prakt. Grammat. d. osman.-türk. Sprache*, 1869.

          C.   Affirmat.: *sew-il-dir-mek* 'to cause one to be loved.'
                Negative: *sew-il-dir-me-mek* 'not to cause one to be loved.'
                Impossib.: *sew-il-dir-e-me-mek* 'not to be able to cause one to
                                                  be loved.'

Reciprocal.   A.   Affirmat.: *sew-ish-mek* 'to love one another.'
                Negative: *sew-ish-me-mek* 'not to love one another.'
                Impossib.: *sew-ish-e-me-mek* 'not to be able to love one another.'

          B. · Affirmat.: *sew-ish-il-mek* 'to be loved by one another.'
                Negative: *sew-ish-il-me-mek* 'not to be loved by one another.'
                Impossib.: *sew-ish-il-e-me-mek* 'incapable of being loved by one
                                              another.'

          C.   Affirmat.: *sew-ish-dir-mek* 'to bring it about that they love each
                                              other.'

          D.   Affirmat.: *sew-ish-dir-il-mek* 'to be so influenced that they love
                                              one another.'

Reflexive.   A.   Affirmat.: *sew-in-mek* 'to rejoice' (lit. 'to love one's self').
                Negative: *sew-in-me-mek* 'not to rejoice.'
                Impossib.: *sew-in-e-me-mek* 'not to be able to rejoice.'

The points to be noticed in this table are the regular system, from which there is no deviation, and the simplicity resulting from the fact that no suffix is loaded with double or triple meaning, as is so frequently the case in the Indo-European languages.

Contrast also the number of Indo-European cases with the carefully elaborated case-system of the Ural-Altaic group.[1]   In these languages we have the following scheme :

A nominative (Suomi : *käsi*) ; a partitive (*kättä*) similar in its use to the French partitive in *boire du vin ;* a genitive (*käden*) also used, in the singular, as objective[2] ; an inessive (*kädessä*, 'in the hand') ; an elative (*kädestä*, 'out of the hand') ; an illative (*käte[h]en*, 'into the hand') ; an adessive (*kädellä*, 'at the hand') ; an ablative or separative (*kädeltä*, 'away from the hand') ; an allative (*kädelle*, 'toward the hand') ; an abessive (*kädettä*, 'without hand') ; a prola-

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[1] The examples are from Boller, *Die Declination in den finnischen Sprachen*, p. 15 f. = *Sitzungsber. Wien. Akad. (philol.-hist. Classe)*, XI (1853), p. 965 ff.

[2] Ujfalvy and Hertzberg, *Grammaire finnoise* (1876), p. 74, separate the objective from the genitive in the singular as well as in the plural.

tive (*kädetse*, 'along the hand') ; a translative (*kädeksi*, expressing a change into the object, or, as terminative, the purpose of an action or limit of time, e. g. 'to translate *into English*,' '*dono* mihi dedit,' 'to postpone *till next year*') ; an essive (*kätenä*, expressing condition, predicate relation, and, as temporalis, time when, e. g. *ihmis*[*e*]*nä*, 'as a man,' *on ilosena*, 'he is glad,' *huomenna*, 'in the morning') ; a comitative (*kätene*, 'together with the hand'), and an instrumental (*käden*, 'by means of the hand'). The careful elaboration of such systems as these, the exact distribution of the main relations possible between members of a sentence over an adequate number of distinct suffixes, is in marked contrast with the easy adaptation of a small number of suffixes to a great variety of usages, which characterizes the Indo-European languages.

It may be added also that there appears to be a much closer correspondence in the case-systems of the various Ural-Altaic languages than exists in the Indo-European. On this point, however, we do not feel able to speak with certainty and must content ourselves with quoting Boller's statement (p. 73) that 'for the inessive, elative and illative, for the adessive, ablative and allative, for the abessive, factive, terminative, temporal and instrumental, all [Ural-Altaic] languages either exhibit a common exponent or, where they have lost it, have created a new sign.'

§ 13. Now in the few cases where we have in historical times within the Indo-European group well-attested instances of semantic agglutination (pp. 75, 78), we find precisely the same symmetry and systematic regularity which we have just seen in the Ural-Altaic group. The negative voice in Old Norse is agglutinated out of the verbal theme, the post-positive personal pronoun (-*k*- for -*ek* in the first person, -*þu* in the second) and the enclitic negative (-*a*, -*at*, -*að*). Thus first sing., *naut-k-a*, 'I partook not' ; *lek-k-að* for **lét-k-at*, 'I let not.' Sometimes with a redundant -*k*, *ma-k-a-k*, 'I cannot.' In the second sing., reversing the order of negative and pronoun, *grát-at-tu*, 'weep not' ; *gaft-at-tu*, 'thou gavest not.' So that even in these sporadic instances regularity and system appear to be the necessary accompaniment of agglutination. But for the great bulk of Indo-European inflections the contrast between the cases just cited and the irregular and unsystematic mass of inflectional material is at once apparent and has often been

pointed out.   Thus Misteli says :[1] 'In contrast to the Ural-Altaic and
Dravidian languages, where the exponents of the plural and those of the
cases are always nicely distinguished and the same sign for a case is
always used in both numbers, the Indo-European terminations of the
genitive plural, *-ōm,* and of the locative plural, *-su,* permit of no analysis
nor of any connection with the corresponding singular cases. . . . The
Ural-Altaic and Dravidian languages retain their logical arrangement
through all periods, as far back as we can trace them, because they
reflect a mode of thought which does not abbreviate and contract its
operations.[2]   The Indo-European did not think highly of pedantic
analysis and separation, if only the whole had its distinguishing mark ;
he forms words, not compounds. . . . The personal endings are by no
means more regular or more logically devised.   The *s* of the ending of
the first person plural *mes* is not, as we have seen, the plural sign.   The
termination of the second person plural *-te,* in which Sanskrit, Greek,
Latin, and Slavic agree, shows plainly that there was no thought of
turning singular endings into plurals.   Strange irregularities, like the
Greek *-μην* or *-thās*[3] in Sanskrit as secondary endings, not to mention
other instances, could hardly be imagined as occurring in an "agglutina-
tive " language.'   In a similar spirit G. v. d. Gabelentz[4] contrasts the
strict logic in the paradigms of the agglutinative languages with the
irregularities and vagaries of the Indo-European inflection, suggesting
the term 'Defectivsystem' for the latter type.

Other passages of the same import might be quoted[5] ; they all repeat

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[1] *Charakteristik der hauptsächlichsten Typen des Sprachbaues* (1893), p. 535 ff.

[2] It will be clear from what preceded and from what follows that we do not agree
with this interpretation.   Misteli's words imply that the Indo-Europeans gave up a
division which they had made.   The formal evidence is all the other way, and points
to the fact that they did not make this analysis and division until much of their para-
digmatic material was finally fixed.

[3] A trace of this ending is probably to be seen also in the so-called passive aorist in
Greek *ἐ-δό-θης* (Wackernagel, *K. Z.* XXX, p. 302).

[4] *Sprachwissenschaft*², pp. 351–2, especially on p. 352, 'Mit Recht rühmt man
den eigentlich sogenannten agglutinierenden Sprachen grössere logische Folgerichtig-
keit nach als den indogermanischen : jede grammatische Kategorie hat dort in der
Regel nur einen einzigen lautlichen Ausdruck und die Wortfolge pflegt durch feste
Gesetze geregelt zu sein.'

[5] Jespersen (*Progress in Language*, 1894, p. 67) : 'Thirdly it may be objected to
the [agglutinative] theory that, assuming it to be true, we should expect much more

what Ludwig[1] urged thirty years ago: 'If the grammatical forms [of the Indo-European] owed their *origin* to necessity [that is, if they had been used, from the beginning, because a need was felt of expressing semantic relations by formal means] and were not simply due to a process of *adaptation* [that is, had not acquired a secondary meaning which was inflectional], then the character of the Indo-European inflectional system as well as that of its word-formation would not be so entirely different from the monotony and systematic regularity of the agglutinative languages.'

It is perhaps unnecessary to accumulate evidence on this point; the facts speak for themselves. But classical philologists, quite naturally and properly, fix their attention habitually upon the regularities and systems that are actually found in Greek and Latin inflection and, in so doing, fail to give sufficient weight to the fact that they are of limited range and are not at all in harmony with one another. It is true that comparative morphology, assisted by our fuller knowledge of the phonetic transformations of the various languages, has been successful in unravelling the snarled skein of Indo-European inflection. But, even so, an unbiased observer can recognize little real system in our paradigmatic architecture. If we look at the case-endings of nouns in the singular, we find, it is true, a single type instead of the old five 'declensions,' but it is for the singular only and is not without internal irregularities. On the other hand, the plural does not repeat the system of the singular,[2]

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regular forms than we actually find in the old Arian languages; for if one definite element was added to signify one definite modification of the idea, we see no reason why it should not have been added to all words in the same way; as a matter of fact, the Romance future, the Scandinavian passive voice and definite article [i. e. examples of agglutinative formations in modern Indo-European languages] present much greater regularity than is found in the inflection of nouns and verbs in old Arian.' Cf. also Cauer, *Grammatica Militans*, 2d ed., p. 67: 'Und zwar hat ihm [Wundt] die Sprachwissenschaft selber im voraus recht gegeben, indem sie, das Wiederspiel von Lautgesetz und durchbrechender Analogie verfolgend, zu einer immer bunteren Mannigfaltigkeit einander durchkreuzender Tendenzen gelangt ist, die sie in jedem, auch dem ältesten Zustande einer Sprache zu erkennen weiss.' Also Morris, *On Principles and Methods in Latin Syntax* (1901), pp. 52–3: 'Looked at broadly, the most striking characteristic of Latin inflection is that it is not a system, but is on the contrary highly unsystematic.'

[1] *Agglut. od. Adapt.*, pp. 23–24.

[2] This would hold good even if Schleicher's analysis of the accusative plural in *-ons*

as would be the case in a truly agglutinative language, but has its own
quite distinct system, without any sign for the plural number (distinct
from the case-signs), with no repetition of the case endings of the
singular and with its own peculiar set of exceptional irregularities. And
all this, again, is different from the case-system of pronouns, which have
their own partially systematized set of suffixes, differing in singular and
in plural and appended to several different stems. In short, as a glance
at the useful tabulation in Brugmann's *Kurze Vergleichende Grammatik*,
II, pp. 373–413, will show, the so-called system of case-endings is not
a system at all, but consists of some half dozen little sets of endings,
each wholly or mainly distinct from the others, no one of them really
complete or full and some markedly defective and irregular.

The inflection of the verb is even less systematic. Forms like the
Homeric κέκλυθι, δή-δεκ-το may belong to the present or to the perfect
system[1]; thematic forms (*bhárati*, φέρω, *eō*) run parallel to athematic
(*bhárti*, φέρτε, *it*)[2]; a Greek form identical with the Latin *habē-s*,
habe-t, and Gothic *habai-s*, *habai-þ*, begets an entirely new 'tense-
system,' namely, the so-called 'strong aorist passive,' e. g., ἐ-μάνη-ς,
ἐ-μάνη[-τ][3]; the same formative -*s*- which makes the 'perfect' *ges-s-ī*
makes also the 'desiderative present' *quaes-(s)-o*.[4] And it is literally
true that this list might be extended to cover pages.

If the irregularity of formal inflection is great, the irregularity of
meaning is greater. The different Indo-European languages, as we
have them and apart from any hypothesis as to a common case-system,
vary considerably in the number of their cases and, therefore, in the
range of meaning attached to each case.[5]   Quintilian[6] felt this so

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as being a compound of the accusative singular in -*om* and the plural sign -*s* were
firmly established.

[1] Brugmann, *Kurze Vergl. Gram.* II, 479, § 641 (end).

[2] *Ibid.*, p. 498, § 646.

[3] Hirt, *Handbuch d. griech. Laut- u. Formenlehre* (1902), p. 366, § 419.

[4] Sommer, *Handbuch d. lat. Laut- u. Formenlehre* (1902), p. 626.

[5] Cf. Sweet, *Words, Logic, and Grammar* in *Transactions of the* [*London*]
Philological Society 1875–6 (published in 1887), p. 477: 'Inflections often express
the same idea in totally different ways' which he illustrates by the divergence in the
English plural forms *dogs* (-*z*), *hats* (-*s*), *men, feet.*

[6] Quaerat etiam sitne apud Graecos vis quaedam sexti casus et apud nos quoque
septimi.   Nam cum dico 'hasta percussi,' non utor ablativi natura, nec si idem
Graece dicam, dativi.   (*Institut. Orat.* 1, 4, 26.)

strongly that he considers the advisability of adding a new case to the
Latin and to the Greek paradigm to ease the ablative and the dative,
respectively. The Greek genitive and dative have large uses which the
same cases in Latin do not have, and both languages are obliged to
make use — but not a like use — of prepositions to supplement the
imperfections of their case-forms. Within a single language the genitive
and the dative are used indiscriminately (after *similis*), the genitive and
ablative (of quality) with but occasional discrimination, and the distri-
bution of genitive and ablative in *quanti emo* and *magno emo* is purely
conventional.[1] The Cyprian and Arcadian dialects show a dative-locative
form after ἐξ and ἀπό (Cyprian ἀπὺ τᾷ ζᾷ, ἐξ τᾷ πτόλιϲι; Arcadian
ἀπὺ τοῖ ἱεροῖ, ἐϲ τοῖ ἔργοι) where the other Greek dialects have an
ablative-genitive form.[2] The genitive and the adjective (*regis, regius*)
may have precisely the same meaning, and a function like *purpose* may
be expressed by a preposition with the accusative, by a supine, by a
gerundive, by *causa* with a genitive, by a future participle, by an in-
finitive, by a *qui* clause, or by an *ut* clause. Nor do Cauer's eight
divisions,[3] good as they are, exhaust the possibilities of expressing
conditional thought in Latin. This is not system. Nor can the word
be more properly applied to what is called the 'tense system.' The
present tense may be used of the past, of the future, or without tense
signification; the imperfect indicative may be durative or conative, that
is, may express shades of meaning that are not temporal at all; there is
a potential use of the future; the distinction in Latin between the present
subjunctive and the perfect is not a tense difference, and it is doubtful
whether the perfect can properly be called a tense. In fact, the dis-
tinction between mode and tense is now seen to be neither an original
difference nor one which permits of clear definition.

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[1] Cf. furthermore Servius (on Verg. *Aen.* I, 176) who speaks of *antiqua circa
communes praepositiones licentia* and contrasts *conditus in nubem* (*Georg.* I, 442) with
conduntur in alvo (*Aen.* II, 401), *sedens super arma* (*Aen.* I, 295), with *super
arbore sidunt* (*Aen.* VI, 203); similar remarks are found in his notes to *Aen.* I, 47
(*tota nocte legi = totam noctem legi*); I, 599 (ablative and genitive with *egere*); II,
713 (ablative and accusative with *egredi*), etc. — For an interchange of the ablative
and instrumental in Sanskrit cf. Pischel, *Gött. Gel. Anz.*, 1884, p. 513; *Zeit d. deut.
morgenl. Ges.* XLII, p. 303; Geldner, *Ved. Stud.* II, (1897), p. 32.

[2] Brugmann, *Griech. Gramm.*[3] (1900), p. 437, § 498, and p. 440, § 500.

[3] *Grammatica Militans*[1], p. 133 = 2d ed., p. 140 ff.

To all this should be added the surprising semantic mobility or
complexity of the Indo-European suffixes.   When we find the attitude
of the speaker (mode), the number, the person, and the time relation,
all four together, expressed by the *-em* of *amem*, and the same combi-
nation of ideas expressed also by *-am* in *moneam* and by *-im* in *sim*, or
the gender, number, case-relation, and an implication of the nominal
character of the word all carried by the single *-i* of *domini*, it is apparent
that the Indo-European formatives are freighted with a heterogeneous
weight of meaning which gives them a very peculiar character.[1]   If
terminations of such semantic complexity are measured by the standard
of the simple systems of suffixes in a truly agglutinative language (see
above, p. 81 ff.), it seems to us impossible to doubt that the two types are
essentially and inherently different.   No conceivable amount of linguistic
decay and fusion can have so completely obliterated the marks of an
originally agglutinative structure.

§ 14.   There appear to be but two possible paths leading toward the
solution of the important problem of semantic inflection ; either the
meanings have from the very start been attached to the suffixes or
they have become associated with the terminations in a long process of
language-growth.   That is, we have to choose between the hypothesis
of semantic agglutination and some form of the hypothesis of semantic
adaptation ; there is no third possibility.

The two essential points upon which a hypothesis must shed light
are, first, the origin of the varied inflectional material and, second, the
strange combination of lack of systematic plan with partial and limited
uniformities.   It is not to be expected that a theory which has met
with but little acceptance as a working hypothesis in morphology or
syntax should be supported by any considerable amount of detailed
work ; it still awaits that testing and possible support.   But some
suggestions may nevertheless be made as to the way in which it
would account for the origin and present condition of Indo-European
inflection.

To the question of origin the hypothesis of adaptation returns an
answer which at first sight may seem unsatisfying and even evasive.

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[1] Cf. Oertel, *Lectures on the Study of Language*, pp. 289–293, § 8, where references
to earlier literature are given.

That answer is that inflectional endings are not essentially different from
word-building suffixes, but are rather to be regarded as word-building
suffixes in a new rôle and partially systematized into paradigms.   This,
to be sure, seems merely to push the question of origin a little further
back, but, on the one hand, it will appear in the next section that the
hypothesis does not wholly shirk the question of origin, and, on the other
hand, the real beginning of inflection comes at the point — wherever in
the long course of development that point may be — where the endings
of two or more different forms of a word begin to be felt to be the
carriers of case relations.   In other words, inflection is not properly a
matter of forms, but of meanings, and that theory which accounts for
the meanings and for their association with forms explains inflection,
whether it accounts for the forms or not.

It is of course clear that at all times certain semantic relations must
have existed between the words that go to make up a group.   They
may have been tacitly assumed by the speaker and readily supplied by
the hearer, but they do not become grammatical entities until they are
formally expressed, either by phonetic or by tactic means [1] (by position).
'The [2] relations and concepts which were to be definitely expressed by
inflection must have existed ever since men began to think and speak.
But only through expression by means of inflection [3] did they become
grammatical categories.'   'I believe [4] it is universally admitted that
there was a time when inflection did not exist.   This belief, however,
is not really valuable until it is supplemented by the second thesis that
there was a time when [what is at present] inflection was not yet inflec-
tion.'   This last thought Ludwig repeats a little further on: 'There [5]
is no absolute and original difference between word-formation and inflec-
tion; the latter is nothing but a development of the former.'   Delbrück
also, writing in 1880, [6] did not look with disfavor upon the general idea of
identifying case terminations with word-formatives.   After lamenting the
hopeless condition of almost all etymological analyses of the case-endings
he continues: 'It may be held either that case endings were added

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[1] Cf. Stöhr, *Algebra der Grammatik* (1898), p. 66.

[2] *Agglut. od. Adapt.*, p. 109.

[3] This omits the (tactic) expression by position.

[4] *Agglutin. od. Adapt.*, pp. 111–112.

[5] *Ibid.*, p. 115.   [6] *Einleitung*[1], p. 91.

with the original purpose of expressing something similar to our present
case-relations, or that stem-formatives developed into case-suffixes, so
that, e. g., the genitive in *-sya* would be simply a stem with adjective
use.   Curtius held this view concerning some cases, Abel Bergaigne
(*Mém. de la soc. d. linguistique*, 2, 358) for the majority, Ludwig for
all.   I do not see that any theoretical objection could be raised against
allowing a certain amount of free play to both theories.'   But repeated
consideration of the point, Delbrück adds, always forced upon him a
final *non liquet.*[1]

It must be said that the evidence[2] by which Ludwig attempted to
support his thesis was in part erroneous and on the whole unconvincing.
But since that time various investigations in morphology and syntax
have accumulated a considerable amount of material which seems to
support his hypothesis and which is the more valuable because it has
been gathered independently of any theory.

§ 15.   The most important evidence is to be found in Van Wijk's[3]
explanation of the genitive singular and in a paper by Hirt.[4]   We do
not believe that any valid objection can be raised to Van Wijk's hypo-
thesis 'that the nominative singular of consonantal stems differed from
the genitive singular in accent only,'[5] that the genitive relation in primi-
tive Indo-European was expressed by position,[6] that the *nomen rectum*
was more strongly accented than the *nomen regens*,[7] that this accentual
difference between *nomen regens* and *nomen rectum* produced formal
differences,[8] and that as the formal differences increased a functional

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[1] On the floating boundary line between inflection and word-formation, cf. also
R. M. Meyer, *Klassensuffixe*, in *Paul und Braune's Beiträge*, XXII, p. 548.

[2] *Agglut. od. Adapt.*, pp. 113–118.

[3] *Der nominale Genitiv singular im Indogermanischen in seinem Verhältnis zum
Nominativ* (1902).

[4] *Indogerm. Forsch.* XVII (1904), pp. 36–84.   It should be remarked also that
Fay published ten years ago a series of interesting and noteworthy suggestions along
this line in the *Amer. Journ. of Philol.* XV (1894), pp. 409–442, and XVI (1895),
pp. 1–27.

[5] *Der nom. Gen. sing.*, p. 5.

[6] *Ibid.*, p. 79.

[7] *Ibid.*, p. 80; a similar accentual difference is seen in Hebrew.

[8] *Ibid.*, p. 81; the same process is seen in the Hebrew formal distinction between
'construct' and 'absolute' cases.

difference between the two grew up, so that finally the old expression of the genitive relation by position was abandoned and supplanted by the two "cases." The chain of reasoning is here so well supported by phonetic evidence that Brugmann[1] singles out this case to illustrate the fact 'that what appears to us as a "case" need not have had this function at the very beginning.' The formal identity of genitive singular and ablative singular except in the *o*-declension is equally patent; so is the identity of form between certain nominatives (neuters in nasals or liquids, and in -*s*) and locatives singular, such as nom. sg. neut. *dur-mánas* 'evil minded' and locat. *sa-dívas* 'at once'; nom. sg. neut. *ū́dhar* 'water' and locat. *áhar* 'by day,'[2] to which may be added parallel forms with *dehnstufe*, such as nom. sg. ποιμήν and locat. δόμην (Cretan infinit.), nom. sg. δώτωρ and locat. νύκτωρ.[3] On the strength of Hirt's 'Basentheorie'[4] a considerable number cf parallelisms may be discovered, which, without straining any phonetic laws, lend support to Ludwig's theory. That the Greek subjunctive forms with short 'characteristic vowel' are formally identical with the indicative forms of thematic verbs was hinted at by Tobler,[5] and fully established by Curtius,[6] who also called attention to the parallelism of the verbal theme *tuda* in *tuda-ti* and the nominal theme *tuda* in accus. *tuda-m* (cf. Greek [ἔ-]μολο-ν and [αὐτό-]μολο-ν). Since there is almost no room for any doubt that the Italic, Keltic and Slavic subjunctive themes in -*ā* are simply heavy bases, the probability is very strong that the corresponding forms in -*ō* and -*ē* are of the same type,[7]

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[1] *Kurze Vergleich. Gramm.* II, p. 418, note to § 529.

[2] Compare Brugmann, *Kurze Vergl. Gramm.* II, p. 379, § 459, 2 and 4, with p. 384, § 466, 2 and 3.

[3] Brugmann, *Griech. Gramm.*³, pp. 228–9, § 262; p. 360, § 424 B, 1.

[4] We agree with Brugmann (*Kurze Vergl. Gramm.* II, p. 490, § 633, Anm. 1) that the question whether the *ē* in *plē* is due to *formal* agglutination or not (Hirt, *Ind. Forsch.* XII, 213) cannot be definitely decided (see above, p. 72). But this question does not affect the present argument.

[5] *Zt. f. Völkerpsychol. u. Sprachwissenschaft,* II (1862), p. 35.

[6] *Zur Chronologie der indogermanischen Sprachforschung,* p. 42 = *Abhandlungen d. K. sächsischen Ges. d. Wissensch.* (*philol.-hist. Cl.*) V (1867), p. 226. His attempt to regard the "union vowel" as the exponent of continued action is, of course, untenable, cf. Ludwig, *Agglut. od. Adapt.,* p. 26.

[7] Phonetically the case is not so clear because both -*ō* and -*ē* might be due to contraction, Brugmann, *Kurze Vergl. Gramm.* II, p. 552, § 717, Anm.

and the first of these suggests again connection with the nominal themes
of the $\bar{a}$-declension.[1] And, if the boundary line between nominal and
verbal formatives is thus seen to be indistinct, is it too far a cry from
the nominal suffix $i\bar{e}:\bar{\imath}$ (Brugmann, *Kurze Vergl. Gramm.* II, p. 350,
§ 429), to the verbal suffix $i\bar{e}:\bar{\imath}$ (Brugmann, *l. c.*, p. 555, § 726 A) ; in
other words, from the nom. sg. *faciē-s* to the second person 'future'
faciē-s and to the 'subjunctive' *s-iē-s*,[2] or from the sigmatic noun-
form *gener-a* (**genes-a*) to the sigmatic verb form $[\acute{\epsilon}\text{-}]\tau\acute{\epsilon}\lambda\epsilon\sigma\text{-}a$, or from
the *-i* which forms the 'nominative' **qo-i* Lat. *quī* to the *-i* which forms
the 'locative' Doric $\tau\epsilon\text{-}\hat{\imath}[\text{-}\delta\epsilon]$?

It is not necessary to go more fully into the details of such corre-
spondences. They are sufficiently treated by Hirt in the paper referred
to above, and for us the important point is not whether this or that
form is rightly interpreted, but whether there is a body of forms con-
siderable enough in number and importance to give support to Ludwig's
hypothesis. It seems to us that the evidence is of such a character, and
that, thus supported by independent evidence, the theory that the bulk
of old Indo-European inflectional terminations was originally used for
word-formation and acquired inflectional meanings at a later stage,
deserves renewed attention and offers to investigation a promising
working hypothesis.

We do not, however, claim — and we would make this statement
expressly and definitely — that instances of agglutination like the Oscan
locative húrtín[3] were absolutely wanting at that early time, but only
that such formations are not typical of the primitive Indo-European
inflectional system and that the inflectional force of the bulk of old
Indo-European formatives is secondary (cf. pp. 68, 75, 98).

§ 16. The exact process by which suffixes were adapted to uses
different from those to which they were originally put is not discussed
by Ludwig in detail. He only alludes to a fading of the original mean-
ing, an emptying of the suffix of its first signification.[4] Many of the

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[1] Cf. for this and the nominal use of the *-ē* and *-ō* forms Hirt, *Handbuch d. griech.
Laut- und Formenlehre* (1902), p. 233, § 306, 1; p. 423, § 480.

[2] Cf. Lindsay-Nohl, *Die Lat. Sprache* (1897), p. 398; v. Planta, *Gram. d. osk.-
umb. Dial.* II (1897), p. 56, concerning nominal and verbal themes in *-ē*.

[3] For **hortēn* from **hortei-en*, i. e., locative **hortei* + preposition *en*.

[4] It seems worth while to notice that Otto (*I. F.* XV, p. 9), for instance, operates

changes of meaning probably did not arise with the speaker, but with the hearer, who carried into the suffix a meaning derived from various other sources. This process is like the change in the meaning of words described by Jaberg,[1] where the real cause of change is a partial misinterpretation by the hearer, who takes a word in a sense not intended by the speaker. In this way arises, for instance, the modern German *während des Krieges* out of a wrongly divided genitive absolute, *währendes Krieges*[2]; so the dative plural *Schwab-en*[3] was interpreted as a nominative singular, first, no doubt, in a context where both were possible and speaker and hearer disagreed; so the plural suffix *-s* of 'all-ways' has become an adverbial termination.[4] But whatever the

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with such colorless suffixes: 'The following investigation confines itself entirely to Greek and Latin. It analyzes forms within these languages only. Hence it is entitled to assume "meaningless suffixes" and need not inquire after their original starting point, because during the period here treated and in the languages here dealt with these suffixes no longer rooted in their original soil (*vom Boden ihrer Entstehung losgelöst*) and were used as purely formal elements.' Cf. also Lenz, *Die neueren Sprachen*, VIII, 1900–1, p. 470, who, though holding to *formal* agglutination, sees the essence of inflection in the adaptation of empty suffixes.

[1] *Die pejorative Bedeutungsentwicklung im Französischen*, in *Zt. f. roman. Philol.* XXVII (1903), p. 37.

[2] Wunderlich, *Der Deutsche Satzbau*[2] (1901), I, p. 393; II, p. 194.

[3] Johansson, *Bezzenb. Beitr.* XIII (1888), p. 111.

[4] It is at this point, in the discovery of traces of connection between word-formatives and inflectional suffixes, that much detailed work is needed to support or — if it shall so result — to weaken the theory. Two general principles appear to be clear. *First*, the line of connection is to be found in the meaning of the word; the meaning of the termination came in part from the word and context. *Second*, it must be remembered that paradigms do not always start with nominatives and presents; the problem is quite as likely to be to find how a certain dative or accusative got its nominative as to find how a nominative acquired a dative form.

A speculative illustration is perhaps permissible.

Suppose a group of words of strongly colored meaning, temporal or locative, used in a vaguely adverbial way, 'in the cave,' 'on the hill,' 'in the forest,' and employed by a group of speakers with distinct reference to particular spots in the neighborhood. Such words would be likely to have similar terminations. So used the words would be locative adverbs and the termination would be the result of the common locative meaning, but would not itself have a meaning apart from the stem. Inflection would begin when a speaker wished to use the word as the subject of a sentence, 'the cave is large,' 'the forest lies in that direction.' For this he would give to it the kind of termination he was accustomed to employ with other words in the nominative; he

exact manner was by which a certain set of old word-formatives assumed
the new rôle of inflectional suffixes, if it be once granted that in the
majority of cases the force with which we find them charged in the his-
torical languages was an acquired force, then the actual uses of a suffix
in this latter period cannot be utilized for the reconstruction of its
primitive meaning. 'As[1] soon as inflection began to prevail, i. e., as
soon as the terminations of the members of a sentence were identified
with the part these members played in the economy of the sentence, so
that their rôle no longer needed to be inferred from the context only,
the original meaning of these terminations became immaterial.'   And
since each suffix acquired its new meanings under varying conditions,
attached to stems of widely varying signification and surrounded by
widely different contexts, these new functions must, from the very
beginning, have been numerous.   Instead of a sprouting of many mean-
ings from a common trunk[2] we shall rather find a process of gradual
consolidation, a tendency from multiplicity toward unity.   We must,
however, not be understood to claim that *all* historical usages of a case
or mode are primitive and can be traced back directly to original
differences.   There are undoubtedly instances in which a given meaning
of a case or mode developed in historical times and after its inflectional
force had been clearly established, exactly as we find words develop
semantically.   Thus it seems to be clearly established that the use of
the imperfect subjunctive as an irrealis of the present, though it was
not yet general in the time of Plautus, became soon afterward so fixed
as to form a well-marked usage and to influence later meanings and
constructions.   In such a case it is, of course, clearly permissible to

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would construct a nominative form by analogy; the strictly locative (case) force would
then become attached to the former adverbial termination and a paradigm would be
started with a locative case and a nominative.   Similarly it may be questioned whether
the 'emotional' modes (optative, subjunctive, imperative) do not really antedate the
'intellectual' mode (indicative).   In this connection Meumann's observations (*Die
Sprache des Kindes*, Zürich, 1903, p. 53 ff.; cf. also Michels, *Germania*, XXXVI,
1891, p. 126) seem to us very suggestive.

    [1] Ludwig, *Agglut. u. Adapt.*, p. 111.

    [2] Fay, *Agglut. and Adapt.* in *Am. Jour. Phil.* XV, 411: 'The Greek genitive is
regarded as a sarcophagus in which bones of dead cases repose.   From the biological
standpoint it ought to be regarded as the representative of an undifferentiated embryo
out of which the differentiated cases have arisen.'

speak of the derivation of this later meaning from the earlier, in fact, investigations of this character form a necessary and most important part of historical syntax. But what we mean to say is that, after all such genealogical relationships have been established, there will remain a residuum — much larger than is at present generally assumed — which permits of no further reduction, and the members of which are to be regarded as independent and primary. We do not consider it possible to derive these from a common source.

§ 17. The answer of the hypothesis of Adaptation to the second question mentioned above, namely, what has caused the partial regularity and system in inflections, seems to us, considering the difficulty of the problem, fairly satisfactory. It is that the degree of system which we find and which has been interpreted as the fragmentary survivals of an earlier and more complete system, is better accounted for if we regard it as the result of a struggle to bring order out of a partially chaotic condition by analogy changes and other means, such as analytical inflection (*du père, j'ai aimé*) and perhaps occasional agglutinations. For this we refer again to Misteli's[1] characterization of the Indo-European family. 'Mechanical forces . . . brought forth a wealth not only of graded root-variants but also of stems and forms which mind was called upon to utilize ; by elimination of what is not useful, and by systematically grouping the rest, by unification and separation, sense is brought into this heterogeneous mass. This activity in transforming and utilizing the speech-material, which for mechanical reasons was disintegrated and varied, continues to the present time and, even in English and Modern Persian, has not reached its limit. Such a struggle we observe in no other linguistic type discussed in this book. . . . Other types display very enduring systems of grammatical categories, which outlived even the change of phonetic material and are continually reproduced in new forms. The Indo-European type does not know any such fixed system ; it continually transforms and develops its categories ; it shows progress, evolution.'

§ 18. It is worth while to emphasize this contrast between inflecting and agglutinative languages alluded to in the passage just quoted because this whole classification rests upon a tacit rejection of the agglutinative theory for the Indo-European languages. Ludwig clearly saw that a

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[1] *Charakteristik*, etc., p. 510.

real distinction between 'inflecting' and 'agglutinative' languages can-
not be maintained except upon the theory that the processes of creating
their grammatical systems were essentially different. 'If,' he says,[1]
'grammatical terminations [in Indo-European] owed their function to
their primary meaning and had not acquired it by this process of adap-
tation, then the character of the Indo-European languages would not
be so very essentially different from . . . that of the agglutinative lan-
guages. It is impossible to close one's eye to this patent contradiction,
that the comparative philology of to-day . . . on the one hand separates
the Indo-European languages, as genetically different, from the agglu-
tinative languages, and yet explains the origin of word-formation and
inflection of the former as due to a process of [semantic] agglutination,
establishing thus a difference which is, at best, quantitative only. . . .
For we cannot escape this dilemma: Either the Indo-European lan-
guages rest upon [semantic] agglutination; in that case the assumed
difference between these inflecting languages and the agglutinative type
falls to the ground. Or they do not rest upon [semantic] agglutination.
In that case their word-formation and inflection must have resulted
from something else than [semantic] agglutination. But since, as
everybody admits, these formatives were at one time added to the
roots,[2] it necessarily follows that when they were added to root or stem
they did not possess the meanings with which we find them charged
later.' 'This[3] repeated semantic transformation [= adaptation] of the
phonetic [raw] material which closely unites the inflectional elements
with the stem and brings about a fusion of the two, this continual
re-coinage of old phonetic material by stamping it with new semantic
values, marks the essential difference between the agglutinative and the
inflectional type.' How Bopp's agglutinative hypothesis removes the
distinction between the two linguistic types is well illustrated by a
quotation from Boller's paper on the declension of the Ural-Altaic
languages, in which he disputes the exclusive claim of the Indo-European
languages to the terms 'declension' and 'case.' 'There are those,'[4]

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[1] *Agglut. od. Adapt.*, p. 24.

[2] Cf., for this assumption of *formal* agglutination, above, p. 69 ff.

[3] *Agglut. od. Adapt.*, p. 28.

[4] *Die Declination in den finnischen Sprachen*, p. 3 f. = *Sitzungsber. d. Wien.
Akad.* XI (1854), p. 953 f.

he says, 'who are opposed to the use of the term "declension" except in connection with inflecting languages. . . . The claim, as it seems to me, is unjust. . . . In order to confine the term "declension" to the inflecting languages it would be necessary to establish a valid criterion by which the case-mutations of the inflecting languages, or what we call declension, might be distinguished from the expressions of nominal relations within the so-called agglutinative languages. Up to now, no one has, to my knowledge, succeeded in doing this. To be sure, they refer us to the *fundamental* difference between *organic development from within* and the *mechanical, purely chance accretions (Anklebung) from without*. But they cannot deny or explain away the fact that — as Bopp has first shown and Pott has again emphasized — the oblique case-terminations [in Indo-European] are really prepositions or rather post-positions. . . . But as soon as one is forced to admit that the formal elements of declension once had independent value (*selbständige Geltung*) there is no gain in this make-shift assumption of an "organic union." For if by this "organic union" we simply mean a unification which is brought about and determined by the accent, then no one who is familiar with the facts would claim that such unification did not exist, especially in the western group of the Ural-Altaic languages, where it is furthermore emphasized by the law of vowel-harmony and syllabic rhythm. In fact, Kellgren, from this point of view, was perfectly justified in classing Suomi among the inflecting languages. . . . As soon, however, as this arbitrary boundary line [between inflecting and agglutinative languages] is removed, the unity of the principle which determines the grammatical expression of nominal relations [in both the inflecting and the agglutinative types] at once clearly appears, and all these formatives or case-signs range themselves in a series which begins with mechanical composition (*Zusammenrückung*) and ends with the complete absorption and assimilation [of the formative] by the nominal root.' Similarly, G. v. d. Gabelentz,[1] accepting Bopp's hypothesis for the Indo-European languages and regarding Indo-European inflection as nothing but a very advanced stage of agglutination, concludes: 'So[2] sind wir wieder um einen Dualismus ärmer, d. h. um eine wissenschaftliche Einsicht reicher.' There is really only this alternative. Either

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[1] *Die Sprachwissenschaft*[2], p. 345 ff.   [2] *Ibid.*, p. 351.

we have to give up the distinction between inflecting and agglutinative languages, or we have to assume that the majority of Indo-European inflectional endings owe their semantic contents to a process of adaptation which is as typical for this family as the process of semantic agglutination is typical for the other group.   This would not necessarily mean that cases of semantic agglutination might not occur in early Indo-European, or cases of adaptation in the Ural-Altaic and Dravidian languages,[1] but only that such cases are not typical for these languages. So, when we speak of a language as having a musical accent, we do not thereby mean that it absolutely lacks all stress-variation, nor does the term 'stress-accenting' imply a complete absence of all pitch-variation. We simply mean that in the former case stress, in the latter pitch, plays a subordinate part and is not typical for the accentual system of these languages.

§ 19.   The effect of the shift of opinion which we have thus far been following and discussing will apparently be to change somewhat radically our point of view with regard to various questions of semantics, and especially of syntax.   Some of the questions which it raises are too broad to be entered upon here, but one in particular cannot be avoided. How does all this affect our attitude toward the *Grundbegriff*, toward the hypothesis of an early fundamental meaning, from which the meanings of inflectional forms in the single languages may be thought to be derived?

§ 20.   The terms *Grundbegriff*[2] and *Grundbedeutung* we owe to Delbrück.   In his paper on the Indo-European, especially the Vedic, Dative (*Kuhn's Zeitschrift*, XVIII (1869), p. 99), he gives the following lucid explanation of the words: 'The term *grundbedeutung* is as important as it is ambiguous, and it needs, therefore, a careful defini-

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[1] Similarly, Delbrück does not exclude the process of adaptation from the Indo-European; cf. *Der Gebrauch des Conj. und Optat.* (1871), p. 19: 'Schon bei der ersten Dualis und Pluralis ergiebt sich die Beobachtung, dass etwas, was ursprünglich nicht in dem Modus liegt, durch die Einwirkung der umgebenden Situation in ihn eindringen kann.'

[2] There is no technical term in English which quite corresponds to these and we shall venture occasionally to use the German words as explained by Delbrück in the quotations given here and below.

tion. Attempts to reach the original signification (*ursprüngliche bedeutung*) of cases, modes and tenses have proceeded along various lines. The first method was that of philosophical construction. This is to-day abandoned, and it is now no longer necessary to refute those who believed that only a limited number of cases is possible and that every actual use had to find a place in the scheme. A second method of tracing the original significations[1] (*grundbegriffe*) is suggested by comparative morphology, viz., to analyze a form into its component parts and determine by means of the etymology the meaning which the form may have. Nothing could be said against this method if the condition of our etymological knowledge permitted such an interpretation of case-forms. But, unfortunately, we know exceedingly little about the elements by which cases are formed from themes, and thus are, as a matter of fact, unable to utilize etymology for the interpretation of syntax.[2] It follows that we are restricted for our purpose to what we can observe in the actual use of cases. One way[3] to derive the original meaning (*eigentlicher sinn*) of a case from its usage was to place the different instances side by side and arrange related uses into a conceptual group with increasing circumference and decreasing concreteness. The result was a logical scheme which was capped off by some such general category as "causality" or "reciprocal action" or the like. It may be possible to discover in this way a very general category for several cases (though not for all), but the understanding of the linguistic processes is not helped thereby, since of course it cannot be supposed that the speakers of a given people had any such

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[1] This is the *absolute Grundbedeutung;* see below, p. 104.

[2] Cf. above, pp. 72–3.

[3] Compare with the following the discussion in *Der Gebrauch des Conjunctivs und Optativs* (1871), pp. 11–12: 'The question which interests us here, viz. "How may the original meaning be derived from the actual uses?" can, it would seem, be decided in one way only. In trying to understand the development of both a verbal, nominal, or pronominal idea and of an inflectional form the principle is the same, viz., not to place the historical uses side by side, to fuse related ideas into more abstract concepts and thus to ascend gradually to the widest and emptiest abstraction, which, as source of all the rest, is to form the pinnacle of the logical scheme, but to find the historical starting point of the semantic development (i. e. the oldest meaning to which the others joined themselves according to the laws of semantic growth), rather than one that will logically embrace them.'

philosophical generalizations in their mind when they used their cases.[1]
It is therefore not permissible to derive the original meaning (*grund-
begriff*) of a case from its uses except in the same way in which one
traces the fundamental meaning (*grundbedeutung*) of a word, viz., along
historical lines. The problem is not to find an abstract concept logically
broad enough to embrace the individual uses, but the historical starting
point whence the semantic development proceeded. In many instances
this point may be reached with the help of either direct or indirect
tradition. In the case of others recourse must be had to general
experience, which teaches that abstract concepts generally arise out of
concrete percepts, not *vice versa*. The most concrete and sensuous
meaning must generally be considered the oldest.

If the uses of a case are viewed in this light, we shall be able to
discover a few concepts — under the most favorable circumstances a
single concept — from which the rest may be derived. This earliest
attainable meaning of a case — no matter whether it consists of one or
several concepts — we term its original meaning (*grundbegriff*). It is
not claimed that this meaning belonged to the case at the time when
the case-form was made, but simply that we do not now succeed in
penetrating further into the history of the case than these concepts or
this concept.'[2]

§ 21. The relation between the *Grundbegriff*, as thus defined, and
the theory of agglutination, formal and semantic, is evidently close, and
the doubts which we have expressed above as to the general hypothesis
apply with equal force to this particular deduction from the theory.
But, aside from general considerations, there are special reasons for
accepting only with reserve, and on the clearest evidence, the idea of
an early fundamental meaning, which is to be the common source of all
later meanings.

The *Grundbegriff* is in fact a semantic reconstruction, parallel to the
formal reconstructions of Indo-European words. It seems to us that
this is a point where the distinction between form and meaning (see

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[1] Cf. Ludwig, *Agglutinat. od. Adaptat.*, p. 38, § 18.

[2] Cf. *Neue Jahrb. f. d. Klass. Altherth.*, V (1902), p. 326, ' . . . anderseits
versuchen wir von ihnen rückwärts zu wenigeren Gruppen oder zu einer Einheit zu
gelangen.'

p. 68 f.) is especially necessary, and that a meaning cannot be established as Indo-European except upon the basis of strictly semantic evidence. The character of the semantic evidence should be precisely similar to that of the formal evidence required to make out a case for the Indo-European provenance of a form. What such evidence should be Delbrück has pointed[1] out with admirable clearness in a review of Fick's *Wörterbuch der Indogermanischen Grundsprache*. 'A form,' he says, 'may be regarded as Indo-European either upon external (historical) or internal (grammatical) grounds. By Indo-European parent speech we understand the language which was spoken immediately before the separation[2] of peoples. With periods of this language which lie back of this date we are not here concerned. Our interest centres in its division into the separate languages. Unfortunately, however, the chronology of this separation is very obscure. . . . All hypotheses are far from certain. Until this question is definitely settled, one thing must be borne in mind : During the whole period extending from the first separation to the beginnings of history new words and forms may have arisen which may be older than the individual languages and younger than the parent speech. Until we know the Indo-European genealogical tree better, we must regard the testimony of each one of the main branches as equally important for the construction of an Indo-European form. Especially are we opposed to the establishment of Indo-European forms upon a basis of parallel forms in the Greek and Indic alone. For who can be sure that they do not belong to a period of Greco-Indic union,[3] and are not therefore some thousand years younger than the real Indo-European period. But may not the want of historical proof be perhaps counterbalanced by the force of grammatical reasoning? Suppose a form (e. g. a verbal form) could be based on two languages only, and suppose, further, that it were so clear in its composition, so complete in its necessary details, so primitive in its appearance and the arrangement of its elements that any one familiar

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[1] *Kuhn's Zeitschrift*, XVIII (1869), p. 74.

[2] If written now, this and some parts of what follows would be differently expressed. But the change in view does not affect the argument here.

[3] We should now say ' of contact between Pre-Greek tribes and Pre-Indic tribes.' Cf. Kretschmer, *Einleitung in d. Gesch. d. griech. Sprache* (1896), chapters IV to VI, especially pp. 168 ff.

with Indo-European grammar could rightly claim : Our language ever
since it became inflectional must have possessed this form, — might not
this internal evidence be counted as equal in weight to the historical?
I feel sure that with the progress of our science this latter method may
be increasingly used.   But I submit that for the present we are not at
all agreed on what the necessary elements of a form are, we still dispute
concerning the original form of the suffixes, and the term ' original '
itself is still vague. . . . Since, therefore, the grammatical method
cannot at present be safely used in all cases, it is but rarely able to
supplement deficiencies of the historical method.'

By substituting the word ' meaning ' for ' form ' in the foregoing
passage we obtain a safe rule by which meanings may be established
for the Indo-European period.   Now, as far as the ' grammatical ' sup-
port is concerned, no one will claim that its value has increased
since Delbrück called attention to its limitations in 1869, while much
of the historical evidence is precisely of the character against which
Delbrück protests ; that is, it is based upon semantic distinctions found
in only a few of the existing languages.   Thus the separation of sub-
junctive from optative, which is discussed more fully below, is based
upon the semantic distinction made only in Sanskrit and Greek.   The
possibility of error in this and similar instances is the result of a failure
to discriminate sharply between formal evidence and semantic evidence ;
forms of both modes are found in a sufficiently wide distribution to justify
the reconstruction of Indo-European forms (the starred forms of com-
parative morphology), but, in order to reconstruct an Indo-European
meaning for these forms with equal certainty, it is necessary that the forms
should have the same meaning in the languages in which they occur.
Otherwise, if, for example, forms occurring in four languages have the
same meaning in only three, the semantic evidence is cut down by just
so much ; the semantic reconstruction (the *Grundbegriff*) is based upon
three languages, instead of four, with the added difficulty that the pecu-
liar meaning of the fourth language must in some way be accounted for.
This by no means inconsiderable gap in our reasoning we have been
prone to overlook, taking it for granted that form and meaning go
together.

§ 22. The same silent identification of the *semantic* and the *formal*
aspects and proofs has been one of the most dangerous pitfalls for the

'linguistic palaeontologists.' Whether ἵππος in Greek is the wild or the domesticated horse we are able to learn from literary and archæological evidence. But what the exact meaning of the Indo-European *eku̯o-s (reconstructed on the basis of all Indo-European languages except the Slavic) was, no one can tell. What little information, therefore, we can gather as to the domestication of the horse in Indo-European times rests solely upon palaeontological evidence. The name of the pig[1] is the same in every Indo-European language. But from literary and archæological sources we learn that the Hindoos and Iranians did not know the domesticated pig, while in Western Europe its domestication goes back to the earliest times. On the other hand, the goat was known to all the Indo-Europeans as a domestic animal, yet its name is represented by not fewer than four separate words.[2] Upon the basis of *formal* identity we should be inclined to assign the beech-tree to the flora of Greece, yet southern[3] Greece does not possess this tree, and the name φηγός stands therefore for the oak (*quercus Aegilops*) and chestnut (*castanea vulgaris*). In a similar way the Latin *quercus* is identical in form but not in meaning with the Old High German *forha*. Such examples as these, and, indeed, the whole history of the attempt to reconstruct the Aryan civilization on philological evidence, must be taken as so many cautionary signals. For the determination of the *Grundbegriff* of a mode is precisely parallel to the reconstruction of the elements of the pre-historic culture, only far less concrete and more liable to error. If we give due weight to these warnings, then the existence in the different languages of the same meaning (the jussive, the locative, the goat) does not justify the conclusion that it was associated with a single form in the parent-speech, nor does the existence of a common form (e. g. such as might be reconstructed for 'pig') justify us in inferring a common *Grundbegriff*. Plainly, the semantic reasoning needs the extremest caution.

§ 23. But scientific caution and the candor which accompanies it have not been lacking, and to the same eminent scholar to whom we owe the introduction of the term *Grundbegriff* we owe also some

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[1] Schrader, *Reallexicon, s. v.* 'Schwein.'

[2] Schrader, *Reallexicon, s. v.* 'Ziege.'

[3] Schrader, *Reallexicon, s. v.* 'Buche.'

important limitations and modifications of it. In 1871 Delbrück again had occasion to define the term, and in so doing he drew an important distinction between the 'absolute' and the 'relative' *Grundbegriffe*.[1] 'Literature,' he says, 'presents nothing but a mass of individual facts. The subjunctive forms appear to express now will, now expectation, now simple futurity, now exhortation; in like manner the optative forms express now desire, now modest assertion, now condition. . . . We look for an answer to the question: What lies at the bottom of such multiplicity of usages? . . . How may the *Grundbegriff* be derived from actual usage? . . . This problem[2] may, in our case, be approached in two ways. Either an attempt may be made to discover by the help of etymology the meaning which these forms had when they first arose, or one may try to ascertain from the actual use of these modes in Greek and Sanskrit literature which is the oldest historical employment of the subjunctive or optative.' The former he calls the 'absolute,' the latter the 'relative' *Grundbedeutung*. An illustration of the former is Curtius' identification of the optative *i* with the root *i*, 'to go,' and his interpretation of φερο-ι-μι as 'I go to carry.'

§ 24. The history of opinion in regard to these two different definitions of the *Grundbegriff* has been different, and is worth following down to the present time. Toward the 'absolute' *Grundbegriff* three attitudes are possible: (1) That of the agglutinative hypothesis, which holds that the *relative Grundbegriffe* and the actual uses in the historical languages are direct descendants of such *absolute Grundbegriffe*. Consequently, as there is a continuous line of development uniting the two historical extremes, it is not, theoretically at least, impossible to reconstruct these primary meanings from those in actual historical use. (2) The attitude of the 'Adaptationstheorie' of Ludwig, which maintains that the suffixes, when they were first added to the roots in word-formation, had demonstrative force.[3] Consequently 'they did not originally modify the meaning of root or stem, but were purely deictic in character.'[4]

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[1] In *Der Gebrauch des Conjunctivs und Optativs*, 1871, p. 11.

[2] *Ibid.*, p. 12.

[3] *Agglut. od. Adapt.*, p. 25: 'dass ich die suffixe nicht als participielle bildungen betrachte, sondern aus bedeutungsumbildung der demonstrativbedeutung erkläre.'

[4] *Agglut. od. Adapt.*, p. 27: 'Die suffixe modificieren ursprünglich die bedeutung von wurzel und stamm gar nicht. Sie gaben beziehungen nach aussen.'

'Or,' he asks, 'does any one suppose that a *rāj-* was called *rāj-an-* because he ruled for a long time, or the earth *bhū-mi-* (for earlier *bhū-*) as geographical knowledge extended?'[1] But this deictic force gradually faded,[2] the suffixes ceased to express external relations, and began (in the 3d period) to derive new meanings from the roots with which they had become united.[3] Suffixes thus 'in the course of time became estranged from their original significations'[4] and came to serve purposes essentially different from those for which they had originally been intended. (3) Jespersen's hypothesis of the 'sentence-word' or 'sound-*continuum*' implies, of course, complete denial of all *absolute Grundbedeutungen*, since it does not recognize even formal composition.

These three attitudes are, however, so far as semantic history is concerned, reducible to two. For Ludwig's and Jespersen's hypotheses agree in holding that the historical uses of the suffixes are not original or inherent, but were acquired by, or grafted on, the suffixes, which according to Jespersen never had any other meaning, while according to Ludwig they had lost it. The hiatus in Ludwig's third period, which separates the actual employment of the suffixes from their original deictic force and severs all historical connection between the two, links his theory to that of Jespersen, and puts both in sharp contrast to the agglutinative hypothesis. Although, as we shall show below, this difference is of great importance for syntactical method, yet with reference to the knowledge of early meanings actually attainable, the theoretical interest which attaches to the contrast is inversely proportional to its practical value. Comparative grammar seems, at present, more firmly than ever committed to the position which Johannes Schmidt[5] outlined in 1881. And since it is uniformly admitted that we do not and cannot know[6] the origin and etymology of the large

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[1] *Ibid.*, p. 26.

[2] *Sitzungsber. Wien. Akad.* LV (1867), p. 134, quoted above; and see the table, 3d Period.

[3] *Agglut. od. Adapt.*, p. 26: 'je ältere Bildungen man aufsucht, desto allgemeiner tritt die erscheinung hervor, dass das suffix die bedeutung nicht modificiert, vilmehr die bedeutung dem stamme, der wurzel entlehnt.'

[4] *Ibid.*, p. 26.

[5] *Kuhn's Zt.* XXIV, p. 320, quoted above, p. 66.

[6] To the quotation from Delbrück's article in *Kuhn's Zt.* XVIII, p. 99, given above, p. 99, and those on p. 73, may be added Brugmann, *Kurze Vergl. Gramm.*

mass of Indo-European formatives, the road toward the discovery of
absolute Grundbedeutungen which, as Delbrück pointed out, rests upon
this very etymology, is effectually barred, even to those who hold to the
theory of semantic agglutination.   And it is worthy of note that as
early as 1871, when Curtius' analysis and etymological interpretation
of the modal stem-formations seemed infinitely more plausible and
acceptable than now, Delbrück devoted but a single page[1] to the
discussion of the *absolute Grundbedeutungen* of both modes, closing
with these words[2]: ' However, these *absolute Grundbegriffe* of ' motion
toward ' [for the optative] and of ' duration ' [for the subjunctive] lie
beyond all literary evidence.   They will play no part in the investiga-
tion which follows.   All I intended here to do was to point out that the
relative Grundbegriffe may be easily derived from them.'

§ 25. While the attempt to follow up the absolute *Grundbegriff* thus
lands us in a necessary agnosticism, the idea of a relative *Grundbegriff*
has, by reason of the more abundant *data*, been more suggestive.   In
the *Conjunctiv und Optativ* it still appeared possible to arrive at an
actual unity (will, wish) as the fundamental meaning of each mode.
There was, therefore, no awkwardness in the use of the term *Grundbe-
griff*, with its rather distinct implication of conceptual unity.   The diffi-
culties, however, in the way of arriving at a single concept which could
be regarded as the semantic source of all later meanings have been
increasing, and in various instances it has been necessary to rest content
with several meanings,[3] related, no doubt, to each other, but still too far
separate and independent to be easily reduced to unity.   Such a group
was still for a time called a *Grundbegriff*, because the points of contact
between its members were emphasized.   But the awkwardness of the
designation is apparent and in more recent discussions other terms,
which do not so distinctly imply unity and which seem to lay less stress

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II, § 365, p. 284: ' von den urindogermanischen Suffixen sind nur wenige in etymo-
logischer Hinsicht nicht hoffnungslos unklar.' Delbrück, *Einleitung in das Studium
der indogermanischen Sprachen*[4] (1904), p. 134: ' und kann es nicht unnatürlich
finden, wenn viele Sprachforscher erklären . . . lieber auf jeden Versuch einer
Deutung der Flexionsformen verzichten zu wollen.'

[1] *Der Gebrauch des Conjunct. und Optat.*, p. 14.

[2] *Ibid.*, p. 15.

[3] See passages quoted above, p. 100 with note 2.

upon inherent meaning and more upon usage, have been employed. These terms — *Anwendungsgebiet, Gebrauchssphäre, Gebrauchsumfang, Gebrauchstypus*, to use the German without translation — are beginning to be used as technical terms, though they still lack, perhaps, something in precision. They promise to be useful and the sense in which they are used may be made sufficiently clear by a quotation or two. 'In[1] the historical languages we discover nothing but types of usage (*Gebrauchstypen*) in which the meaning of the case seems to depend wholly on the situation. The speaker is not in even the slightest degree conscious of a *Grundbedeutung* of a case. . . . If now we compare the types of usage (*Anwendungstypen*) in the oldest historical periods of the individual languages, we may with considerable certainty reconstruct for each case the chief types of usage in Indo-European, such as the genitive with verbs of control, the partitive, the subjective, the objective genitive, the ablative with verbs of motion, with comparatives, etc.' And below (p. 335), with reference to the modes, 'By a type of usage (*Gebrauchstypus*) of the subjunctive I understand a state of things where an intimate association of this mode with a definite complex (*Konstellation*) of thought and feeling has taken place,' such, for instance, as the deliberative or anticipatory subjunctive. Brugmann also uses similar terms in a passage (*Kurze Vergl. Gram.* II, p. 417, § 529) which illustrates both earlier and later uses, and which we quote without translation: 'Der aus den einzelnen Gebrauchstypen sich zusammensetzende Gebrauchsumfang, den die Kasus in derjenigen Zeit der idg. Urgemeinschaft hatten, in die wir durch Vergleichung der verschiedenen idg. Sprachen zunächst zurückkommen, ist das, was man unter ihrer Grundbedeutung zu verstehen hat.' Both scholars are careful to say that they would now lay less stress upon the original or fundamental meaning and more upon the detection and interpretation of types of usage in the single languages.

§ 26. With the idea of a *Grundbegriff* thus relegated to the background and the 'type of usage' brought to the front, it might well seem that all actual and practical differences between the two theories were at an end. To show that this is not the case, it is worth while to review the points of agreement and of difference.

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[1] Delbrück, *Die Grundbegriffe der Kasus und Modi*, in *Neue Jahrb. f. d. Klass. Altert.*, V (1902), p. 325.

So far as the primitive meanings of inflectional forms are concerned both theories are agreed in acknowledging that they cannot, in our present state of knowledge and perhaps ever, be recovered ; the steps by which we are brought to this confession of ignorance matter little. As to types of usage also there is considerable agreement. Both theories hold that such types exist in the historical languages and constitute the largest semantic unities actually existing. Both admit that some of them may be more recent than others and may have been developed genetically from earlier types of usage. Both would say that upon sufficient semantic evidence types of usage might be proved to be pro-ethnic, though the tendency of the agglutinative view would be to magnify the probability of early types, which the hypothesis of adaptation would in its turn equally tend to minimize. But here agreement ends and differences begin — differences not as to the existence of types of usage, but as to their nature and their place in the scheme of inflectional development and their consequent importance in syntactical work. These appear to us to be so fundamental as to call for extended discussion and illustration.

§ 27. As viewed in accordance with the agglutinative hypothesis, ' types of usage ' such as the subjective genitive or the potential are not to be considered merely convenient abstractions ; they are groups of related uses so bound together by association that, though the speaker did not define the subjective or the potential idea, he yet felt their likeness of meaning. They are, therefore, in a sense, entities, realities, unities, and, as such, they may be regarded as standing at the beginning (so far as our *data* carry us) of a line of development. From such types of usage all later uses are derived by various differentiations and shifts of meaning. The subjunctive, starting from the concept of will, by the gradual retirement of the dominant element of will and the increase of the element of assertion and futurity, develops the many shades of meaning of which it is capable in historic times. The process is comparable to the branching of a family tree ; the uses of a form are connected genealogically, as the members of a large family are connected through a supposed common ancestor. If, then, we take up a mass of syntactical material, starting with the hypothesis that the various uses are thus related, the object of the investigation is to determine the degree of relationship and the line of connection with the

head, and the method is that of detecting the resemblances in meaning, closer or more remote. The investigator thus endeavors to reconstruct the genealogy. He selects typical cases and reasons upon resemblances.

This is, certainly, a perfectly legitimate method of procedure, provided the original working hypothesis, that the relation of the minor uses to the 'type of usage' is genealogical, be correct. But if we are forced to think that this hypothesis is incorrect, that the general process of growth of meanings is in no proper way comparable to a family tree, then the method should also fall with the hypothesis upon which it rested. We cannot then be satisfied with reasoning by typical cases and resemblances in meaning.

§ 28. The hypothesis that the meaning of inflectional forms was acquired by a process of adaptation likewise recognizes 'types of usage,' but its understanding of their place in the scheme of inflectional development is quite different, and, being at once more complex and less familiar, it requires more detailed explanation.

In attempting a reconstruction of the earlier periods of Indo-European speech the twin doctrines of *gradual growth* and of *original local differences* are of prime importance. Upon these Ludwig insisted as fundamental to his *Adaptationstheorie:* 'Since[1] inflection is not the creation of a moment, and since it did not transform the whole of pre-grammatical syntax at once, it must have started from small beginnings and its development must have been gradual, until finally uninflected syntax was supplanted by an inflectional system.' 'If[2] the subjunctive function is of secondary origin and branched[3] off from an older indicative function, it needs, I hope, no proof that such transformation of meaning was not carried through *at once* and throughout the whole of the Indo-European territory.' It seems to us that current syntactical views pay too little attention to the ethnological and geographical conditions under which those who spoke Indo-European dialects lived. Work in syntax is still too much tinged with Schleicher's belief in a small and fairly homogeneous Indo-European people, and is

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[1] *Agglut. od. Adapt.*, p. 39.

[2] *Ibid.*, p. 78.

[3] Ludwig is speaking here of a thematic subjunct. like *asa-ti* which is parallel to an unthematic indic. like *as-ti*.

only grudgingly willing to assume original dialectic differences.[1]   But
the recent ethnological discussions of Ratzel [2] make it perfectly clear
that for the development of a secondary ethnic group, with such definite
ethnic characteristics as the Indo-Europeans exhibit, we are forced to
assume a very large area ; for its dispersion over a wide area, being its
only protection against alien influences and external interference, was
the sole guarantee of its survival.   The greater the periphery the smaller
is relatively the line of contact.   To think of the tribes inhabiting such
an area as being, at any time, ' united by the consciousness of a common
tongue ' seems to us to imply a complete reversal of all that we know
empirically of political and linguistic history.   For in both the course
appears uniformly to be from multiplicity toward unity.   As all historical
nations are the result of a consolidation of tribes, so all historical lan-
guages are the result of a consolidation and unification of dialects.
What else does the great diversity of the Italic dialects with their
marked divergence in the most common words as well as in their
grammatical material indicate, but that the hordes and tribes which
invaded Italy were far from uniform, and linguistic unification, in this
case, accompanies, almost before our eyes, the political and economic
consolidation of Italy under the leadership of Rome.   If we once admit
that the Indo-European ethnic group was not primary, but long before
the opening of history developed in and inhabited a large area embracing
Middle and Eastern Europe and reaching far into Western Asia, then
the assumption of a well-rounded and evenly-developed grammatical
structure becomes as impossible as that of an uniform culture.   There
is, we believe, practically general agreement that it is not necessary to
assume that all Indo-Europeans at any time possessed knowledge of,
and terms for, agriculture.   The absence of such terms among the
Eastern branch need not be regarded as due to loss ; we are justified
rather in seeing in them primitive differences.   Recently Brugmann has
expressed the belief that all Indo-Europeans did not begin their solar

[1] As early as 1867 Ludwig wrote (*Sitzungsber. Wien. Akad. d. Wiss.* LV,
p. 134) : ' Auch die Grundsprache in der Zeit ihrer (natürlich nur relativen) Voll-
kommenheit bot keine vollkommene Einheit.   Die Grundzüge der Unterschiede der
spätern Sprachzweige als in der Ursprache bereits vorhanden und wirksam zu ver-
muthen ist nun freilich eine *petitio principii*,' etc.

[2] *Berichte d. sächs. Gesellsch. d. Wiss.* (1898), p. 1, and (1900) p. 25.

year with twelve holy nights of the winter solstice.[1]   And many similar
cases might be cited.   In precisely the same way it seems to us
not only unnecessary but even impossible to assume a fully or highly
developed and generally accepted differentiation of the categories of
mode,[2] tense, and case.   All indications seem to point rather toward a
state of things in which discrimination in meaning and differentiation in
function were much less advanced than we find them in any existing
language, in which, indeed, the distinctions which now seem to us most
obvious, like that between mode and tense or between word-building
suffixes and inflectional terminations, were neither definitely made nor
equally made in different localities.   It cannot be any more correct to
speak of an Indo-European optative mode than to speak of a common
Indo-European agricultural vocabulary.   While we do not, of course,
deny the possibility or the actual occurrence of fusion in the history
of the Indo-European inflectional system,[3] we do hold that many sup-
posed losses and fusions are better regarded as primitive and original
local absences and primitive and original local failures to differentiate.
There is, to our minds, a wide gulf separating the starred, reconstructed
forms of the Indo-European from the results of semantic reconstruction.
Even those who do not believe in the historical reality of the former
regard them as a useful and convenient device, helpful algebraic formulae[4]
by which the comparison of parallel forms in two or more languages may
be most easily and conveniently summarized and expressed.   But as
soon as it is admitted that the function is not inherent in the form,
semantic reconstructions of this kind, so far from serving any good pur-
pose, will often only tend to cloud the issue by injecting foreign semantic
elements which in reality were, perhaps, never present in the history of
a mode, tense, or case, by creating a semblance of well defined system

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[1] *Indog. Forsch.* XV (1903–04), p. 91, note.

[2] See below, p. 119, § 31.

[3] Thus we have said nothing about the ablative, locative, and instrumental; there
are some indications that differentiation and specialization were further advanced in
these cases than in the subjunctive and optative modes.   Cf. on *Kasussynkretismus*,
e. g. Brugmann, *Kurze Vergl. Gramm.*, II, p. 418, § 530.

[4] Delbrück, *Einleitung in das Studium der indogermanischen Sprachen*[4], 1904,
p. 127: 'bequeme und anschauliche Formelausdrücke für Behauptungen, die sich in
Worten nicht ohne Umständlichkeit ausdrücken liessen.'

where in reality there were but tentative and imperfect beginnings, by endowing a case, mode, or tense with a fictitious definiteness of meaning which it never possessed. Schooled as our minds are in logical operations and analytical thinking we are too prone to analyze the semantic content of a form in a manner foreign to the primitive speaker. We are wont to ask, for instance, if the future meaning of a form like Lat. *erit* (for **es-e-t*) *arose out of* the modal meaning.[1] And yet a glance at Jerusalem's[2] discussion of the 'Erwartungsurtheil' will show how intimately the ideas of future time, of volition, and of potentiality are here fused. A truly logical and well defined concept like 'future time' or 'potentiality' requires a very considerable amount of logical acumen and effort; it stands not at the beginning but at the end of a long line of development.[3] This very point was clearly brought out by Ludwig[4] when he tried to show that the force of exhortation, which is found in the first persons plural and dual of the subjunctive, is not a historical development of the idea of simple volition inherent in the first person singular of the same mode: 'For aside from the fact that these two meanings are very closely related, there is no reason why the idea of futurity should not be attached to the first persons singular, dual and plural: so that *vácānsi miçrā́ kṛṇavāvahāi nú* (*RV.* 10, 95, 1) might well be translated: "We two shall now exchange words with one another," which would include exhortation. And if it is claimed that *kṛṇavāvahāi* could not get the meaning "I desire to do and desire that you may do" except because the speaker realized that the idea of volition was really applicable to himself only, while with reference to a second and third person it changed into exhortation, the form is ana-

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[1] Cf. Tobler, *Zt. f. Völkerpsych. und Sprachwiss.* II (1862), pp. 34–5; Bennett, *Appendix to Latin Grammar*, p. 151: 'The so-called Future Indicative of the Third and Fourth Conjugations is (outside of the First Singular) a Present Subjunctive of the *ē*-formation which *has taken on Future function*, e. g., *fer-ē-s, audi-ē-s*,' etc. (The Italics are ours.)

[2] *Die Urtheilsfunction* (1895), pp. 134–9.

[3] Jerusalem, *l. c.*, p. 137: 'Es stellt sich das Bedürfnis ein, das subjective Moment deutlich hervortreten zu lassen, es *entstehen* die Ausdrücke "vielleicht, wahrscheinlich, vermuthlich," die potentielle Wirkung wird als Möglichkeit erfasst und *so die Ausdrucksweise immer exacter*. Es ist jedoch nicht unsere Aufgabe die *feine Nuancierung* und ihren sprachlichen Ausdruck weiter zu verfolgen.'

[4] *Agglut. od. Adapt.*, pp. 76–7.

lyzed in a way which the speakers of that age never resorted to. It is quite possible for us to translate *kṛṇavāvahāi* both by "we two shall make" and "I desire that we two should make," but it must be borne in mind that our translation expresses the matter with a definiteness which the original lacks. The form, no doubt, may be thus analyzed as to its meaning, but this meaning was not at that time expressed with such definiteness in the form. This is the important and decisive point, and it would be absurd to suppose that the speakers at that time rendered to themselves so clear an account of the conditions. The mistake is that the interpretation of the form is based on an analysis which in reality was not performed.' In all investigations of phenomena essentially psychological there is danger that we may be tempted to accept our scientific analysis of a compound phenomenon into its elements as proof that these elements once existed independently and that the compound was formed out of such elements by combination and unification. This is as little true in psychology as it is in biology, a point which is properly emphasized by Jodl[1]: 'The error of all errors in psychology is to suppose that the development of our consciousness is genetically built up out of what an analysis discloses as simple elements. On the contrary we have always an original complex, and what takes place is not the composition of this complex whole out of its constituent parts, but the analysis of the complex into its elements. In life the whole always precedes its parts; only science works in the other direction.' Nor should these unanalyzed complex wholes be regarded in the light of what logic calls 'general concepts.' The two really stand at opposite ends of the line of mental development. General concepts imply analysis, comparison, and the final fusion of the common elements of a number of particular percepts into the 'general concept.' The early complex notions, on the other hand, are characterized by the absence of a well defined boundary within which the idea is comprised and which fences it off against other concepts. We start here with a concrete percept, but this percept is incapable of resisting association with a large number of kindred ideas, and therefore it loses its own identity.[2]

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[1] *Lehrbuch der Psychologie*, p. 177. Cf. also Wundt, *Völkerpsychologie*[1], I, 2, p. 185, and Thurneysen, *Die Etymologie*, p. 34.

[2] Cf. Meumann, *Die Sprache des Kindes* (1903), p. 60 f.

It is a percept with floating boundaries, which does not hold attention fixed on a small and definite semantic area, but allows the mind to rove over a whole range of associated areas. It is not until interest, consequent attention, and the growing power of analytical thinking cut off this comparative freedom of unchecked association and reduce the contents of these unwieldly and alogical[1] aggregates that we obtain the well defined, logical concepts with which the trained mind is accustomed to operate. But to arrive at such simple concepts as that of pure futurity, or the distinction of willing and wishing, requires much maturity of mind and logical skill.

§ 29. The hypothesis of adaptation, then, presupposes in the period before the historic languages had taken shape, a condition of things in which anything like a *Grundbegriff* is inconceivable and in which even well-defined ' types of usage ' must have been rare. On this hypothesis inflectional endings were not wholly distinct from word-building suffixes,[2] and the forces of analogy and assimilation had not yet had sufficient effect to produce anything so distinct and systematic as a mode or a case system ; the lack of system which still characterizes the Indo-European languages was in that early period even more characteristic. Local dialectic differences existed widely and generally. But in this mass of inflectional material the assimilating forces were actively at work and the beginnings of limited systems were already appearing.

With these general statements in regard to the semantic condition of the early period, we are in a position to understand the attitude of those who accept the hypothesis of adaptation toward the ' types of usage.' It is, in general, the reverse of that to which the theory of agglutination would lead. On this hypothesis the types of usage, like the deliberative subjunctive, the subjective or possessive genitive, are not ordinarily to be regarded as the beginnings of a process of semantic expansion and diffusion, but as the end and result of a process of concentration and specialization. Multiplicity did not come out of unity, but unity out of

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[1] Cf. Meumann, *l. c.*, top of p. 64.

[2] As a theoretical illustration the hypothesis regarding the origin of the Latin genitive sg. of *o*-stems may be taken. According to Michels (*Germania*, XXXVI, 1891, p. 133 f.) and Sommer (*Handbuch d. lat. Laut- und Formenlehre*, 1902, p. 371, note 3) the genitive termination -*i* may be identical with the word-formative -*i* (-*iē*- : -*i*-, cf. *reg-i*[-*na*], *victr-i*[-*x*])), and an Indo-European *g̑n̥tis u̯l̥qu̯ī* may have changed from ' wolfish progeny ' to ' the wolf's progeny.'

multiplicity; out of many uses, varied and shifting within a certain range, arose slowly the consciousness of common elements of meaning, by which single uses were bound together into groups, and smaller groups into larger. And the method of investigation which would rest upon such a working hypothesis is also almost the reverse of that which would be followed if the investigation were directed by the agglutinative theory. In dealing with a mass of syntactical material, the attention would not be fixed upon resemblances in order to discover genealogical lines of connection, but upon differences in meaning and use in order to detect and reconstitute the smaller groups out of which the later 'type of usage' was formed; and instead of trying to select typical instances, which would represent as nearly as possible the essential characteristics of a group, the observer would note rather the extremes, the outlying, the exceptional instances, because these would be more likely to disclose those accidental lines of association and analogy by which two smaller groups were afterward united in the consciousness of the speaker into a larger group, the nucleus, perhaps, of a 'type of usage.'

One other point deserves mention, as illustrating both the agreement of the two theories and their essential differences. In a definition of 'type of usage' quoted above (p. 107) Delbrück says, in effect, that in a type of usage two things are combined, an inflectional form (e. g., Germ. *wäre*) and a complex of concepts and emotions expressed in the other words of a sentence (e. g., *wäre ich nur erst zu Hause!*); if either the inflectional form or the setting be lacking, the *Gebrauchs-typus* does not exist. So far as there can be any difference in regard to the definition, it would be a difference in emphasis. To one whose prepossession is in favor of inherent meanings, it will seem that in such a combination the inflectional form was the more important element; to one who, on the other hand, regards the meanings of inflectional forms as acquired through use, such a combination will be interesting chiefly as affording a clue to the way in which the setting affects and ultimately determines the meanings, e. g., of a mode. The difference is chiefly in emphasis, yet it certainly has much to do with methods and even with results.[1]

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[1] Delbrück says that in such a sentence as *wäre ich nur erst zu Hause!* the sub-junctive must play the leading part, since it is essential to the sense, while the particles are not. That certainly is the case, in this particular sentence. But in *quid ego*

It thus appears that, while there are between the two theories points of agreement as to the existence of types of usage and other points where the difference is mainly one of emphasis, there is still a fundamental difference of view in regard to the nature of these semantic groups and, consequently, in regard to their value to the investigator.[1] For if we can continue to hold the belief that such types were the early centers from which later meanings have branched out, then certainly the understanding of them is of the greatest interest, is indeed essential to syntactical progress. But if the main current flows the other way, if the subjective or the possessive genitive is no more than an awakened consciousness of likeness between usages already formed, if *quid ego nunc agam ?* marks the conclusion of a semantic process, then the 'type of usage' is of less interest; it is the process of its formation that is interesting; it has the interest of a product, not the interest and importance of a source. We are driven back upon that alternative which Ludwig long ago expressed : *Agglutination oder Adaptation.*

§ 30. The preceding statements in regard to the attitude of the two theories toward the 'type of usage' are necessarily made in general terms. For a full comprehension of the differences it would be necessary to test the two methods by actual application to a large collection of cases.[2] But it may help to render the point somewhat clearer if we

nunc agam ? — an example of a well-marked *Gebrauchstypus* — the subjunctive is not essential; *ago* is used without difference in meaning; *quid* is more nearly the essential word in this case.

It is, in fact, extremely difficult to determine just how much value the inflectional endings have in expressing thought. The writers, in a spirit not wholly serious, tried upon each other the experiment of writing out a passage of easy Latin without the endings, to see how far it was possible to get the sense and even the construction merely by the order and arrangement. The reader may like to try the experiment. See also the suggestive pamphlet of W. Nausester, *Denken, Sprechen und Lehren, I, Die Grammatik,* 1901.

[1] Their pedagogical value is quite a different matter, which we do not enter upon here.

[2] Examples of investigations based upon the working hypothesis of agglutination may easily be found. Examples of work based on the other hypothesis are of course less common, but we may perhaps mention the thesis of L. D. Brown, on *The Case Constructions of Words of Time* (New Haven, 1904), and the articles on the Subjunctive in Independent Sentences in Plautus (*Am. Jour. Phil.* XVIII), by one of the writers of this paper, though the latter was not consciously predetermined by the theory.

attempt to illustrate by a few selected examples,[1] with the reminder that they are intended for illustration merely, not for proof.

If we take up a collection of genitives dependent on substantives with the hypothesis that they are all descended from some single concept (the ' type of usage ') such as the possessive idea, we shall begin our arrangement by selecting some typical instances in which the idea of possession is most distinct, as τὰ ὅπλα τῶν ἀπὸ τῆς πόλεως Ἀμπρακιωτῶν (3, 113, 2), τρεῖς καὶ δέκα τριήρεις Λευκαδίων (3, 69, 1), πολλὰ μὲν ἐμπόρων χρήματα καὶ σῖτος (7, 24, 2). To these we should add perhaps cases like δοῦλοι ὄντες τῶν αἰεὶ ἀτόπων (3, 38, 5), τῆς βασιλέως στρατιᾶς (1, 93, 7), ἐς Ἄρνισαν πρῶτον τῆς Περδίκκου ἀρχῆς (4, 128, 3). In all these the idea of possession is sufficiently distinct and these would represent the most concrete and therefore the earliest form of the concept of possession. From them, by an easy transition, the idea of possession would be extended to immaterial objects, as in τοῦ πάππου ἔχων τοὔνομα (6, 54, 6), and a little further to Νικίου μὲν ἦν γνώμη (6, 47, 1) and τὰς Ἀθηναίων φύσεις (7, 48, 4) ; in all of these the idea of possession, though slightly shifted, may still be clearly felt. Such a case as τῆς Λακεδαιμονίων πολιτείας (5, 31, 6) may likewise be interpreted as essentially possessive, and the same idea might be extended to τὴν ξυμμαχίαν τῶν Λακεδαιμονίων (5, 25, 1). On the outskirts of the group, representing the widest deviation from the parent stem, would come such cases as περὶ τῶν βασιλέως πραγμάτων (8, 58, 1), τὰ τῶν Ἀθηναίων (4, 79, 2), τὸ Κλέωνος (3, 47, 5). In regard to the last one might hesitate between calling them possessive and referring them to other types, e. g., the subjective. ' The confederation of the Lacedemonians ' might well enough be looked at as the confederation formed by the Lacedemonians. In fact, more than one element of meaning is present and a difficulty in assigning a particular instance to one or another category is characteristic of this method. But the general type is sufficiently distinct and the most direct and presumably the oldest line (τὰ ὅπλα, etc.) is well represented. Taken all together these cases exhibit well enough a particular ' type of usage,' developing

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[1] The examples are all from Thucydides (Hude's text) and are kindly put at our disposal by Miss Palmer, Instructor in Greek in Vassar College, from her forthcoming thesis.

in various directions and yet retaining a general resemblance in all its branches.

The same collection of cases would be approached from a different side by one whose study of them was directed by the hypothesis of adaptation. Starting with the idea that the genitive is merely the case of one substantive dependent upon another, it would be assumed that there is no reason why any two nouns representing concepts capable of standing in a relation to each other should not be so put together. This is the condition of original indefiniteness. The next step is to see that the nature of the relation is the outgrowth of and is determined by the two concepts, the meaning of the two nouns. The grouping of the examples therefore depends upon a classification of the nouns according to their meaning.[1] In the cases here used the governed noun is always a person ($\pi\acute{o}\lambda\epsilon\omega\nu = \pi o\lambda\iota\tau\hat{\omega}\nu$ being only an apparent exception); such groupings as exist depend therefore on the meaning of the governing nouns. Of these we should then make various classes, e. g., names of persons, $\theta\upsilon\gamma\alpha\tau\acute{\epsilon}\rho\alpha$ $\Theta\epsilon\alpha\gamma\acute{\epsilon}\nu o\upsilon\varsigma$ (1, 126, 3), $\delta o\hat{\upsilon}\lambda o\iota$ $\check{o}\nu\tau\epsilon\varsigma$ $\tau\hat{\omega}\nu$ $\alpha\acute{\iota}\epsilon\grave{\iota}$ $\mathring{a}\tau\acute{o}\pi\omega\nu$ (3, 38, 5), $\theta\epsilon o\grave{\upsilon}\varsigma$ . . . $\tau\hat{\omega}\nu$ $\mathrm{^{\prime}E}\lambda\lambda\acute{\eta}\nu\omega\nu$ (3, 59, 2); nouns of personal activities and characteristics, $\tau\grave{\eta}\nu$ $\tau o\hat{\upsilon}$ $\mathrm{B}\rho\alpha\sigma\acute{\iota}\delta o\upsilon$ $\gamma\nu\acute{\omega}\mu\eta\nu$ (4, 123, 2), $\tau\grave{\alpha}\varsigma$ $\mathrm{^{\prime}A}\theta\eta\nu\alpha\acute{\iota}\omega\nu$ $\phi\acute{\upsilon}\sigma\epsilon\iota\varsigma$ (7, 48, 4); nouns denoting customs, laws, rights, $\tau\grave{\alpha}$ $\pi\acute{\alpha}\tau\rho\iota\alpha$ $\tau\hat{\omega}\nu$ $\pi\acute{\alpha}\nu\tau\omega\nu$ $\mathrm{Bo}\iota\omega\tau\hat{\omega}\nu$ (2, 2, 4), $\mathring{a}\nu\theta\rho\acute{\omega}\pi\omega\nu$ $\nu\acute{o}\mu o\varsigma$; nouns of general meaning denoting interests, affairs and the like, $\pi\epsilon\rho\grave{\iota}$ $\tau\hat{\omega}\nu$ $\beta\alpha\sigma\iota\lambda\acute{\epsilon}\omega\varsigma$ $\pi\rho\alpha\gamma\mu\acute{\alpha}\tau\omega\nu$ (8, 58, 1), $\tau\grave{\alpha}$ $\tau\hat{\omega}\nu$ $\mathrm{^{\prime}A}\theta\eta\nu\alpha\acute{\iota}\omega\nu$ (4, 79, 2); names of material objects, $\check{o}\pi\lambda\alpha$, $\tau\rho\iota\acute{\eta}\rho\epsilon\iota\varsigma$, $\tau\epsilon\acute{\iota}\chi\eta$, etc.; and minor subdivisions of all these might be made. Studying these with a little care (a larger number of cases would make it plainer), we should see that the relation expressed varies slightly from one group to another, that it even varies with a variation in the meaning of the same word ($\tau\grave{\alpha}$ $\tau\hat{\omega}\nu$ $\mathrm{^{\prime}A}\theta\eta\nu\alpha\acute{\iota}\omega\nu$, 4, 79, 2; 6, 92, 5; 8, 24, 5; $\gamma\nu\acute{\omega}\mu\eta$, 4, 123, 2; 6, 47, 1; 1, 62, 3), and that when a number of governing words are of substantially the

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[1] In the ablative, also, the meaning of the substantive may affect or even determine the ' function ' of the case. Nouns of a distinct and clearly defined meaning give a clearly defined kind of ablative; an ill-defined meaning of the substantive leaves the ablative also vague; a noun of two or more different meanings has a corresponding number of different ' functions,' that is, the kind of ablative varies as the meaning varies; and the addition of a highly colored adjective often changes entirely the meaning of the ablative.

same meaning, we have a partially fixed and defined function. So we might make a genitive of relationship (with υἱός, ἀδελφός), a genitive of ownership (ὅπλα, τριήρεις, χρήματα), a genitive with nouns of activity, a group with nouns of law and custom, and so on. And undoubtedly, by the fact of continued use, a more or less distinct sense of the similarity of function within each little group would be felt. The genitive of persons with names of objects that could be owned would all be associated together and the relation of ownership would be attached to the genitive as one of its meanings. Thus would be formed a number of small groups, each with its specific function. The next step would come with the perception of a similarity between neighboring groups; the group of ownership and the group of relationship would be connected by such a word as δοῦλοι, the group of customs and laws with the group of activities by γνώμη, and a sort of fusion of groups would begin, through an awakened consciousness of similarities. This would reach its last stage through the fact that the governed noun is in all these cases a person; the relation is therefore personal, that is, one element of meaning is common to all these cases. When this was more or less consciously felt, the 'type of usage' was formed; we call it possessive merely from its most concrete contributing group.

§ 31. A second illustration of the difference in syntactical method that results from the acceptance of the theory of adaptation in place of the theory of agglutination as a working hypothesis may be drawn from the supposed instances of fusion or syncretism of modes or cases. The most striking example is the Latin subjunctive, in which are united the true subjunctive forms (-*ē-s*, -*ā-s*) and the optative forms (*s-iē-s, sīs, faxīs*). So long as we believe that from the beginning the mode, tense and case formatives were charged with a definite modal, temporal and case meaning, inherent in them, a formal reconstruction carried with it the semantic reconstruction also. In other words, if it could be shown that a given suffix-form existed in most Indo-European languages, it followed of necessity that the particular semantic employment of that suffix in some few languages was also to be regarded as established for the pro-ethnic period. It is upon this basis that the assumption of an Indo-European distinction between subjunctive and optative rests. As we find the formative -*iē*- : -*ī*- both in Greek and in Sanskrit charged with an optative meaning, and the formatives -*ē*-, -*ā*-, -*ō*- charged with

a subjunctive meaning, it is assumed that they must have been origin-
ally and in the Indo-European period carriers of the two modal mean-
ings respectively.   And if we find that some one language, like the
Latin, fails to distinguish between $\bar{\imath}$ and $\bar{a}$ forms (*faciās; faxīs*), the
failure is explained as the result of the fusion of two modes originally
distinct.[1]   This is so distinctly the orthodox doctrine that, so far as we
are aware, no one[2] has ever questioned it.   It rests chiefly upon that
silent identification of form with meaning of which we have spoken
above, and this we consider a fatal flaw in the chain of reasoning ;
semantic reconstructions must be supported by semantic evidence.
Nor does a second defect in the reasoning seem to have been dis-
covered, viz., that the modal distinction is found only in Sanskrit[3]
and Greek ; the evidence on the semantic side therefore falls far
short of completeness[4] ; no form would be held to be Indo-European
on the basis of Sanskrit and Greek forms only.   But the slightness of
this proof has been unnoticed because, in fact, the chief reliance has
been upon the formal evidence.   If, however, it be admitted that
the modal meaning of the suffix $i\bar{e}:\bar{\imath}$ (optative) and the long vowel
forms -$\bar{e}$-, -$\bar{a}$-, -$\bar{o}$- (subjunctive) was not from the beginning inherent in
these forms, then there is no reason why we should not regard the
Greek and Sanskrit distinction between the modes as a later differentia-
tion.   The Latin, then, does not present an instance of the fusion of
two modes which the Indo-European distinguished, but of a continua-

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[1] Cf. e. g. *Neue Jahrb. f. d. Klass. Altert.* V (1902), p. 325: 'Ich würde sagen:
in der indogermanischen Zeit, wo es zwei Modi gab, war an den Formen derselben
die Natur der Begehrung mit hinreichender Deutlichkeit erkennbar, im Lateinischen
aber, *wo die beiden Modi zusammengeflossen sind*, muss man diese Natur an der Um-
gebung der Form erkennen.'   And *Der Gebrauch d. Conjunct. u. Optativs* (1871),
pp. 102–3 : 'Das Sanskrit und Griechische haben die Geschiedenheit der beiden Modi
bewahrt, ebenso das Zend und Altpersische, *in den übrigen Sprachen sind die Modi
zusammengeflossen.* . . . . Es ist keinem Zweifel unterworfen, dass das Indogerman-
ische alle die Modi besass, welche das Sanskrit und Griechische zeigen und *dass die
übrigen Sprachen nur verarmt sind.*'

[2] Except one of the writers of this paper, in the *Am. Jour. Phil.* XVIII, p. 392
and p. 395.

[3] Including the Old Persian and the Avestan.

[4] Cf. the general statement on pp. 101–2, § 21.

tion of an undifferentiated condition.[1]   If we start, as we should on the hypothesis of adaptation, with small groups of practically synonymous Indo-European suffixes, the fate of such suffixes must have been the same as that of a small group of synonymous words.   There are three possible ways in which such a group of synonyms may develop[2]: (1) All but one of the synonyms may disappear; for the history of language does not show instances where a spoken language has allowed itself to be burdened with words of identical meaning.[3]   (2) The different synonyms may be utilized as carriers of more or less important differentiations, e.g., German *Rabe* and *Rappe;* such synonymous formal differences have perhaps a tendency to induce semantic differentiation.[4] (3) The synonyms are employed to complete one paradigmatic whole,[5] as the stems *magnifico-* and *magnificent-*, *supellec-* and *supellectil-*, the verbs *furīre* and *insanīre,* and many others.   By dividing up the semantic area they continued to live in spite of their synonymy.   This threefold treatment is illustrated in the history of the modal formatives *iē* : *ī* and the long thematic vowels -*ē*-, -*ā*-, -*ō*-, for all of which we assume substantial identity of meaning in the Indo-European period.   (1) The Germanic branch has practically discarded the forms with long thematic vowel[6] and confined itself to the *ī* suffix.   (2) The Greek and Sanskrit have availed themselves of the formal difference and developed a

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[1] It is not without interest to read in this connection Sayce's article 'The Jelly-Fish Theory of Language' in *The Contemporary Review* for April, 1876 (vol. XXVII, pp. 713–723, especially p. 718) and its discussion by the Rev. R. Morris in the President's Annual Report for 1876 of the London Philological Society (*Transactions of the* [*London*] *Philol. Soc.* 1875–6, p. 277; cf. also *ibid.* p. 16).

[2] Oertel, *Lectures on the Study of Language*, pp. 321–2.

[3] We are inclined to doubt that any of the instances which at first glance might seem to be in violation of this rule (*Athenaeum*, 1902, I, p. 42, Jan. 11) will, on closer examination, prove genuine exceptions. They are either not really synonymous, distinguished, for instance, by their emotional quality, or they belong to different styles, or to different dialects, or they are artificially carried on in the literary language.

[4] Ludwig, *Agglut. od. Adapt.*, p. 74: '. . . abgesehen . . . von der verkerten auffassung, als hätte die bedeutungsverschiedenheit die verschiedenheit der form und nicht vilmer die formverschiedenheit die anknüpfung einer modificierten bedeutung angebant. . . .'

[5] Oertel, *Lectures on the Study of Language*, pp. 181–2.

[6] Brugmann, *Kurze Vergl. Gram.*, p. 551, § 717; Streitberg, *Urgermanische Gram.* (1896), p. 346, § 222.

'subjunctive' and an 'optative' mode.   (3) The Latin, finally, has maintained both kinds of formatives by employing them in one modal paradigm : *fac-s-ī-s, amē-s* (if for *amā-i̯ē-s*[1]), *fer-ā-s, s-i̯ē-s*.   We cannot see why the Greek and Sanskrit adaptation of the *i̯ē* : *ī* formative to the optative use should be regarded in any other light than the Keltic and Italic adaptation of the verb-endings containing -*r*- to passive use. These endings also are found sporadically outside of the Keltic and Italic (e. g., in the Sanskrit secondary endings of the third person plural, -*ur*, -*ran*, -*ranta*, etc.) just as *i̯ē* : *ī* is found sporadically in Latin.   But their adaptation to passive use is a local peculiarity of a certain section of the Indo-European territory, just as the adaptation of the *i̯ē* : *ī* suffix to a special 'optative' mode is, according to our view, a local peculiarity of another section.

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[1] It should be noted that the morphology of this form is not entirely clear; cf. v. Planta, *Gram. d. Osk. Umb. Dial.* II, pp. 299–300, § 306; Buck, *An Oscan Umbr. Gram.*, p. 174, § 232; Sommer, *Handb. d. Lat. Laut- und Formenlehre*, p. 559, § 345.

THE USE OF THE HIGH–SOLED SHOE OR BUSKIN IN GREEK TRAGEDY OF THE FIFTH AND FOURTH CENTURIES B.C.

By Kendall K. Smith

" THEN the centre doors of the dressing-house open and Oedipus comes forth (the first actor). He seems taller than an ordinary man, because he has on the buskin (or tragic boot) and because he is wearing the lofty tragic mask, which rises high above his own head." — Brander Matthews.[1]

Such is the popular conception of a Greek tragic actor. Scholars who have taken a deeper interest in the subject tell us that this buskin had a sole at least four inches high, and often, perhaps usually, reached a height of eight or ten inches.[2] Even the most conservative agree that actors in classic Greek tragedy walked about in shoes with soles of more than ordinary thickness.

In the face of such unanimity of opinion it may seem strange that I have ventured to investigate the subject afresh. It was the doubt expressed by Professor Edward Capps, of the University of Chicago, that first prompted my investigation of the evidence on the use of the buskin or high-soled shoe in the fifth and fourth centuries before Christ. This evidence I found to be both insufficient and contradictory ; but I present it in full in the hope that it may be valuable for reference, even though it lead us to no positive conclusion.

Many difficulties arise in treating a subject of this nature. None of the works of art have been accessible except through publications, and these in many cases, I fear, inexact. For this reason my calculations of the height of different soles is only approximate. Furthermore, I

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[1] *The Development of the Drama*, p. 57, New York, 1903.

[2] Boettiger, *Kl. Schr*. I², p. 283, Leipzig, 1850. Flach, *Das griechische Theater*, p. 24, Tübingen, 1878. Genelli, *Das Theater zu Athen*, p. 84, Berlin and Leipzig, 1818. Donaldson, *The Theatre of the Greeks*⁸, p. 280 ff., London, 1875. Geppert, *Die altgriechische Bühne*, p. 272, Leipzig, 1843; and the more recent authorities.

have not had time to take up the problem of the Roman tragic boot which Dierks[1] says did not have a high sole. Consequently I may have used evidence for the Greek buskin which belonged to the Roman *cothurnus*. In any case, it has not been my purpose to discover the shape or size of the buskin, but simply to find out if the classic period made *use* of the high sole.

EVIDENCE FROM CONTEMPORARY LITERATURE

Two passages from Aristophanes furnish all the evidence from contemporary literature on which can be based arguments in support of the high-soled tragic shoe; and this evidence is extremely uncertain. In *Ranae*, 35, we see Dionysus before the door of Heracles. The latter is surprised to see his visitor in such outlandish garb and exclaims, 45 ff.,

> ἀλλ' οὐχ οἷός τ' εἴμ' ἀποσοβῆσαι τὸν γέλων
> ὁρῶν λεοντῆν ἐπὶ κροκωτῷ κειμένην.
> τίς ὁ νοῦς; τί κόθορνος καὶ ῥόπαλον ξυνηλθέτην;

Heracles is convulsed with laughter because of the absurd dress in which Dionysus appears. The latter is Heracles so far as the lion-skin and club are concerned, but his own cloak and shoes reveal his true identity. The κόθορνος, therefore, was the shoe of Dionysus, and in course of time came to be the tragic actor's foot-gear when the revelries in honor of the Wine-God had developed into the tragic drama. So Crusius[2] argues, rejecting the idea that Dionysus here appears in women's garments. He says, " Mir wenigstens scheint es durch keinen Wink des Dichters angezeigt, dass der Gott nicht in seinem üblichen Prachtgewande, sondern geradezu als Weib vermummt auf die Bühne gekommen sei. . . . Auch die Fussbekleidung der tragischen Schauspieler darf in der Hauptsache als ein *Ueberlebsel aus jener ältern Zeit* angesehen werden; mit andern Worten: der sicher bezeugte κόθορνος des Dionysos war ursprünglich mit dem der tragischen Schauspieler identisch."

If this passage shows the κόθορνος to be strictly the shoe of Dionysus, and retained as such by the later tragic actors, then the second passage

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[1] Dierks, *De Tragicorum Histrionum Habitu apud Graecos*, p. 18, Göttingen, 1883.

[2] Crusius, *Zu den Bühnenalterthümern*, in *Philologus*, XLVIII, p. 702 ff.

from Aristophanes, *Av.* 994, may be fairly assumed to carry on this thought. True, no Dionysus, nor yet one of his followers, is the person wearing the shoe, but the pompous swing of the verses at this point may well herald the entrance of some heroic personage in tragic garb. At these lines we discover the Founder of " Cloud-Cuckoo-Town" busy on his great undertaking, but troubled by all sorts of pestering artists, among them the geometrician Meton. When Meton enters, Peithetairos exclaims, 992 ff.,

> Πει. ἕτερον αὖ τουτὶ κακόν.
> τί δ' αὖ σὺ δράσων; τίς δ' ἰδέα βουλεύματος;
> τίς ἡ 'πίνοια, τίς ὁ κόθορνος τῆς ὁδοῦ;

and Meton replies,

> Με. γεωμετρῆσαι βούλομαι τὸν ἀέρα
> ὑμῖν διελεῖν τε κατὰ γύας.

Crusius (*l. c.*) says of this passage, " Kock[1] bezog den Ausdruck auf *die tragische Fussbekleidung* und übersetzt mit Hemsterhuis *ad quod iter te tam magnifice accinxisti?* Wer ohne Voreingenommenheit an die Stelle herantritt, wird dieser Auffassung einen erheblichen Grad von Wahrscheinlichkeit nicht absprechen; ein Mann, der γεωμετρῆσαι βού-λεται τὸν ἀέρα, kann die tragischen Stelzen gut gebrauchen." 'To measure off the air' Meton needed to approach as closely as possible to the clouds. That is why he put on κόθορνοι and gave poor Peithetairos such a shock.

This rendering of κόθορνος as 'buskin' is the common one, and is doubly attractive since Crusius has shown how neatly and aptly this meaning fits the answer of Meton in the lines that follow. If this be the correct interpretation, we have very good evidence that κόθορνος was the name of a special shoe worn by actors, and we may perhaps be permitted to assume, as Crusius[2] does, that it was of the same general

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[1] Kock, *Kom. d. Aristoph.* IV³, p. 184. This line has been very troublesome to critics and has been variously rendered by all who do not emend the text. E. g. Merry, *Birds* (Notes, p. 54), translates: 'What means this tragic stride (lit. 'buskin') of your coming here?' and suggests that " possibly κόθορνος was a slang phrase for 'swagger.'" Cf. Blaydes, *Aves*, p. 70, 'calceamentum proprie tragicum.' Green, *Birds*, p. 140, 'cur tam superbe incedis?'

[2] Crusius assumes an extremely moderate elevation compared with later times, but yet a considerable sole.

character in the classical period as in the second century A.D., when κόθορνος is distinctly used by Pollux[1] and Lucian[2] of tragic shoes with a very high sole.

But is this the correct rendering? I have presented the argument of Crusius in as unbiased a way as possible. Now, I must ask what is the common meaning of κόθορνος when used not only by Aristophanes, but by other writers of the classical period?

The word occurs thirteen times in classical literature, and it is recorded that one of the plays of the comic poet Philonides was entitled Κόθορνοι. Three times the word is used of a shoe for women alone.

1. Ar. *Eccl.* 344–346,

> μὰ τὸν Διόνυσον οὐδ' ἐγὼ γὰρ τὰς ἐμὰς
> Λακωνικάς, ἀλλ' ὡς ἔτυχον χεζητιῶν
> ἐς τὼ κοθόρνω τὼ πόδ' ἐνθεὶς ἵεμαι.

These words merely repeat what he had said at greater length, 311 ff.,

> τί τὸ πρᾶγμα; ποῖ ποθ' ἡ γυνὴ φρούδη 'στί μοι;
> ἐπεὶ πρὸς ἔω νῦν γ' ἔστιν, ἡ δ' οὐ φαίνεται.
> ἐγὼ δὲ κατάκειμαι πάλαι χεζητιῶν,
> τὰς ἐμβάδας ζητῶν λαβεῖν ἐν τῷ σκότῳ
> καὶ θοἰμάτιον· ὅτε δὴ δ' ἐκεῖνο ψηλαφῶν
> οὐκ ἐδυνάμην εὑρεῖν, ὁ δ' ἤδη τὴν θύραν
> ἐπεῖχε κρούων ὁ κοπρεαῖος, λαμβάνω
> τουτὶ τὸ τῆς γυναικὸς ἡμιδιπλοίδιον
> καὶ τὰς ἐκείνης Περσικὰς ὑφέλκομαι.

The shoes which Blepyros calls κοθόρνω he first designates as his wife's "Persians,"[3] a name suggesting some kind of effeminate oriental foot-gear. They are plainly contrasted with his own ἐμβάδες (Λακωνικαί) which his wife has taken as part of her disguise as a man.

2. It is rare to find such corroborative evidence as is given by a passage from Herodotus, 1, 155, κέλευε δέ σφεας κιθῶνάς τε ὑποδύνειν τοῖσι εἵμασι, καὶ κοθόρνους ὑποδέεσθαι . . . καὶ ταχέως σφέας, ὦ

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[1] Poll. 4, 115; cf. 7, 85.

[2] Luc. *Pro imag.* 3; *Somn.* 26.

[3] Poll. 7, 92, ἴδια δὲ γυναικῶν ὑποδήματα Περσικαί.

βασιλεῦ, γυναῖκας ἀντ᾽ ἀνδρῶν ὄψεαι γεγονότας. Here we find the *Persian* king, Cyrus, advised by Croesus to compel the Lydian men to wear κόθορνοι, and by means of this effeminate foot-gear as well as the effeminate cloaks to turn them into a womanish and unwarlike nation ; 'make them women instead of men.'

3. Equally convincing is Ar. *Lys.* 657–8,

εἰ δὲ λυπήσεις τί με,
τῷδέ γ᾽ ἀψήκτῳ πατάξω τῷ κοθόρνῳ τὴν γνάθον.

If a chorus of women wear it, the κόθορνος is surely a woman's shoe.

Another place where the effeminate nature of the κόθορνος may be practically assured by the company it keeps is the fragment of Lysippus, quoted by Pollux, 7, 89,[1]

βλαύτῃ, κοθόρνῳ, Θετταλίδι.

The other two are expressly the shoes of fops and dandies.[2] It would be unnatural for κοθόρνῳ to occur, as it does, between these two if its style and use were not similar.

It is not difficult to reconcile with this view of the κόθορνος the anecdote about Megacles, the Alcmaeonid, and Croesus, Hdt. 6, 125, κοθόρνους τοὺς εὕρισκε εὐρυτάτους ἐόντας ὑποδησάμενος . . . πρῶτον μὲν παρέσαξε παρὰ τὰς κνήμας τοῦ χρυσοῦ ὅσον ἐχώρεον οἱ κόθορνοι . . . ἐξήιε ἐκ τοῦ θησαυροῦ ἕλκων μὲν μόγις τοὺς κοθόρνους. Even though a man wears them, he has to procure them and wears them because they are large roomy boots and will hold much gold. No contradiction can be found in these words to the other passage from Herodotus (1, 155) which designates the κόθορνος expressly as a woman's shoe or boot. The shape of the boot is what is emphasized by this passage and not the sex of the wearer.

If we compare with this passage the words of Xenophon, *Hell.* 2, 3, 30–31, τὴν δημοκρατίαν μεταστῆσαι εἰς τοὺς τετρακοσίους καὶ ἐπρώτευεν ἐν ἐκείνοις, ἐπεὶ δ᾽ ᾔσθετο ἀντίπαλόν τι τῇ ὀλιγαρχίᾳ συνιστάμενον, πρῶτος αὖ ἡγεμὼν τῷ δήμῳ ἐπ᾽ ἐκείνους ἐγένετο· ὅθεν δήπου καὶ κόθορ-

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[1] Kock, *Com. Att. Frag.* I, p. 701.

[2] A. A. Bryant, *Greek Shoes in the Classical Period,* in *Harv. Class. Stud.* X, pp. 83, 89.

νος ἐπικαλεῖται· καὶ γὰρ ὁ κόθορνος ἁρμόττειν μὲν τοῖς ποσὶν ἀμφοτέ-
ροις δοκεῖ, ἀποβλέπει δὲ ἀπ' ἀμφοτέρων, and the reply of Theramenes,
2, 3, 46, ἀποκαλεῖ δὲ κόθορνόν με ὡς ἀμφοτέροις πειρώμενον ἁρμόττειν,
the style of boot is easily perceived. The κόθορνος must have been a
large, easy boot without 'lefts' or 'rights,' fitted to either foot, and so
the nickname of the 'turncoat' Theramenes. Neither of these last
two passages contradicts what was positively stated before, that the
κόθορνος was strictly the women's shoe or boot.

Let us now turn to an investigation of the passages from Aristophanes
which seem to support the notion of a tragic boot. As before stated,
these passages are two in number ; but a third is inseparably connected
with them : Ar. *Ran.* 556-7,

> οὐ μὲν οὖν με προσεδόκας,
> ὁτιὴ κοθόρνους εἶχες, ἂν γνῶναί σ' ἔτι;

In these words we may have a suggestion both of their size, if they helped
to disguise, and of their unfitness for such a man as Heracles. This is
only a later incident in the story which begins with Ar. *Ran.* 47 (see
p. 124). The argument which Crusius proposes on this passage is that
κόθορνος was the shoe of Dionysus and became in time the tragic actor's
shoe. Twice elsewhere, however, in Aristophanes we find it the natural
shoe for a woman, in *Eccl.* 346 being carefully contrasted to the hus-
band's ἐμβάς. It is reasonable, then, that we should expect Aristo-
phanes to be consistent in his usage and that we should find something
effeminate in the nature of the wearer of the κόθορνοι of the two above
passages. And what else is Dionysus? More effeminate than most of
the goddesses themselves ! Crowned with leaves of the vine ! Stagger-
ing along with the thyrsus in one hand and the cantharus in the other !
A robe of saffron over his shoulders and " Persian shoes " — the product
of the luxurious Orient from which he came — embracing his legs !
Such, I think, must be the picture we have of Dionysus.[1] I admit that
the κόθορνος was the shoe worn by Dionysus, but the reason it was
ascribed to him is that it was in the first place the luxurious woman's

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[1] Such a picture occurs on a red-figured vase in the Louvre, Millin, *Peintures de
Vases Grecs*, I, 9. Cf. Pausan. 8, 31, 4; Lecuyer, *Terres Cuites*, II, pl. P⁵, Paris,
1885.

shoe. Just as κροκωτός was a woman's robe,[1] and therefore assumed by Dionysus, so the κόθορνος, the shoe of women, was assigned to him as becoming his effeminate character. Consequently, this passage only strengthens the theory that the κόθορνος was peculiarly the shoe for women.[2]

The evidence thus far adduced is wholly in support of this theory. The last passage, and the one on which all the argument for the tragic shoe is based, is Ar. *Av.* 994 (see p. 125); and while we find a man wearing the κόθορνος here, it is not a proper shoe for him, as the exclamation of Peithetairos discloses. The question naturally arises, why Meton, the geometrician, should be wearing the κόθορνοι. The answer of Crusius, " Ein Mann, der γεωμετρῆσαι βούλεται τὸν ἀέρα, kann die tragischen Stelzen gut gebrauchen," is ingenious, but both biased and out of harmony with the meaning of the word in all other passages from classical literature. If there had never been in later times a high-soled tragic buskin, such an idea could never have been extracted from this passage. The answer would have been found in the meaning of the word elsewhere, "women's shoe," and this would be sufficient cause for the exclamation of Peithetairos. And why should Meton wear κόθορνοι? Either simply to raise a laugh because of the incongruity of the thing, or because effeminacy was the weakness in Meton's character at which Aristophanes here wished to strike.[3] This weakness has never been brought out, so far as I know. Yet his actions in connection with the expedition to Sicily the year previous, his refusal to go on being drafted,

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[1] Ar. *Thesm.* 138, 253, 945; *Eccl.* 879. Crusius (*l. c.*) calls it the garment of Dionysus, relying on the unconvincing scholium to Ar. *Ran.* 47, κροκωτός. Διονυσιακὸν φόρημα, against Kock who says (*Kom. d. Aristoph.* III[4], p. 44, n. 46), " Der κροκωτός (vgl. K. Herm. *Gr. Privatalterth.* 22, 13) ist ein safranfarbiger Weiberrock (Lys. 44: γυναῖκες κροκωτὰ φοροῦσαι), dessen sich freilich auch verweichlichte Männer zuweilen bedienten, wie selbst bei den Römern Clodius, als er sich unter die das Fest der Bona Dea feiernden Frauen einschlich (Cic. *De harusp. resp.* 21, 44)."

[2] Terra-cottas show this shoe worn by women. *Griech. Terracotten aus Tanagra und Ephesos im Berl. Mus.*, pl. 29, Berlin, 1878. Kekulé, *Griech. Thonfiguren aus Tanagra*, plates, Stuttgart, 1878.

[3] Robert, *Hallisches Winckelmannsprogramm*, 22, 1898, p. 32. " Der Vers der Frösche 47 den Crusius unbegreiflicher Weise zum Beleg dafür anführt, dass κόθορνος im fünften Jahrhundert den hohen Theaterschuh bedeutet habe, beweist gerade das Gegentheil."

his pretended madness and attempt to burn down his house as proof of it, were surely evidences of unmanliness sufficient for a comic poet's jibes. His introduction here, then, in women's boots is not hard to understand.[1]

An impartial study, then, of the word κόθορνος and its use in literature of the classical period gives no authority for the interpretation, tragic boot, or buskin.[2]   Nor are any of the other words so used which gained this meaning in later centuries.   These words are ἀρβύλη, ἐμβάς, ἐμβάτης, κρηπίς, and ὀκρίβας.

ἀρβύλη. — Suidas (and he alone) seems to have taken this word to mean the tragic boot because of its common use in tragedy.   For, of the four words for shoe used in tragedy, κρούπαλον occurs once,[3] εὔμαρις twice,[4] πέδιλον three times,[5] and ἀρβύλη ten times.[6]   This fact is further attested in that it is used by people of all ranks and in unlike situations: by Hera for dancing, Eur. *Her. Fur.* 1303–4,

> χορευέτω δὴ Ζηνὸς ἡ κλεινὴ δάμαρ
> κρούουσ' Ὀλύμπου δῖον ἀρβύλῃ πέδον,

by Agamemnon, returning from Troy, Aesch. *Ag.* 944–5,

> ἀλλ' εἰ δοκεῖ σοι ταῦθ', ὑπαί τις ἀρβύλας
> λύοι τάχος πρόδουλον ἔμβασιν ποδός,

by Pentheus within his palace, Eur. *Bacch.* 636–639 (cf. 1134),

> ἥσυχος δ' ἐκβὰς ἐγὼ
> δωμάτων ἥκω πρὸς ὑμᾶς, Πενθέως οὐ φροντίσας.
> ὡς δέ μοι δοκεῖ, ψοφεῖ γοῦν ἀρβύλη δομῶν ἔσω,
> εἰς προνώπι' αὐτίχ' ἥξει,

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[1] This story is told by Plutarch twice (*Alc.* 17; *Nic.* 13), and by Aelian (*Var. Hist.* 13, 12).

[2] There is no possibility of this meaning left if we emend the line, as Blaydes and Van Leeuwen do.

[3] Soph. *Frg.*, p. 140 (Nauck).

[4] Aesch. *Pers.* 657; Eur. *Orest.* 1370.

[5] Eur. *Elect.* 460; *Frg.*, pp. 529, 655 (Nauck).

[6] Aesch. *Ag.* 944; *Frg.*, p. 83 (Nauck); Eur. *Bacch.* 638, 1134; *Elect.* 532; *Her. Fur.* 1304; *Hipp.* 1189; *Orest.* 140, 1470; *Frg.*, p. 529 (Nauck).   It occurs once outside of tragedy, Hippocr. *De Artic.* 828 D.

and by a chorus of women, Eur. *Orest.* 140–1,

> σῖγα, σῖγα, λεπτὸν ἴχνος ἀρβύλης
> τίθετε, μὴ κτυπεῖτ᾽.

As a travelling boot it appears in Eur. *Orest.* 1470, and *Electra*, 532, and I should agree with Bryant[1] in interpreting Eur. *Hipp.* 1188–9,

> μάρπτει δὲ χερσὶν ἡνίας ἀπ᾽ ἄντυγος
> αὐταῖσιν ἀρβύλαισιν ἁρμόσας πόδας

as meaning the straps or shoe-pieces into which the charioteer slipped his feet when he made ready for driving. What Aristotle understood by ἀρβύλη is seen from a fragment quoted by Macrobius, 5, 18, 20, τοὺς δὲ Θεστίου κόρους τὸν μὲν ἀριστερὸν πόδα φησὶν Εὐριπίδης ἐλθεῖν ἔχοντας ἀνυπόδετον,

> τὸ λαιὸν ἴχνος ἦσαν ἀνάρβυλοι ποδὸς
> τὸ δ᾽ ἐν πεδίλοις, ὡς ἐλαφρίζον γόνυ
> ἔχοιεν,

ὡς δὴ πᾶν τοὐναντίον· ἔθος τοῖς Αἰτωλοῖς. τὸν μὲν γὰρ ἀριστερὸν ὑπο-δέδενται, τὸν δὲ δεξιὸν ἀνυποδετοῦσιν. His equivalent for ἀνάρβυλος is ἀνυπόδετος, showing that he considers ἀρβύλη the poetical word for ὑπόδημα. It is even further identified with πέδιλον, an every-day shoe of the sandal type.

From all these passages we gather that the ἀρβύλη was used by god, by hero, and by chorus for the various purposes of dancing, travelling, and indoor wear, and that it was also the name applied to a part of a chariot. Therefore it is the common general word used by tragedy for shoe. It is perhaps the poetical equivalent for ἐμβάς. Suidas, the sole authority for its meaning 'buskin,' must have been unable to distinguish between the tragic boot, as he understood it, and the name used by tragedy to designate shoes in general.

ἐμβάς. — This word together with its derivatives occurs twenty-six times in literature of the classical period,[2] of which number twenty-three

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[1] A. A. Bryant, *Gk. Shoes in the Class. Period*, in *Harv. Class. Studies*, X, p. 75. With Dindorf, Scholiast, and Eustathius, ad *Il.* 5, 720, p. 599, 22, cf. Daremberg et Saglio, *Dict. d. Ant.*, s. v. *Arbulé*.

[2] Alexis, *Frg.* Kock, II, p. 309. Ar. *Eccl.* 47, 314, 342, 507, 633, 850; *Eq.* 321, 870, 872, 875; *Nub.* 719, 858; *Plut.* 759, 847, 941; *Vesp.* 103, 275, 447, 1157. Eubul. *Frg.* Kock, II, p. 175. Menand. *Frg.* Kock, III, p. 33. Theopomp. Com. *Frg.* Kock, I, p. 748. Hdt. 1, 195. Isaeus, 5, 11.

are in comedy. The ἐμβάς is never worn by women except when they are disguised as men : e. g. Ar. *Eccl.* 46,

<div style="text-align:center">

τὴν Σμικυθίωνος δ' οὐχ ὁρᾷς Μελιστίχην
σπεύδουσαν ἐν ταῖς ἐμβάσιν;

</div>

and is strongly contrasted to the women's κόθορνος, as has been shown (p. 126). ἐμβάδες were worn commonly by old men (*Vesp.* 1157 ff.), at dinners (Eubul. *Frg.* Kock, II, p. 175), in winter (Ar. *Vesp.* 445 ff.; cf. *Plut.* 847), and were a sign of poverty (Isaeus, 5, 11). Never have we any hint of their being used by tragic actors to increase their height. Whatever their shape, they must have been rough shoes for men.

ἐμβάτης. — This is the commonest name for the high-soled boot of tragedy in later literature. Extant literature of the classical period contains ἐμβάτης only once, and then without the slightest connection with the drama. Xen. *De re equestr.* 12, 10, κνῆμαι δὲ καὶ πόδες ὑπερέχοιεν μὲν ἂν εἰκότως τῶν παραμηριδίων, ὁπλισθείη δὲ καὶ ταῦτα, εἰ ἐμβάται γένοιντο σκύτους, ἐξ οἷουπερ αἱ κρηπῖδες ποιοῦνται· οὕτω γὰρ ἂν ἅμα ὅπλον τε κνήμαις καὶ ποσὶν ὑποδήματ' ἂν εἴη. 'As the shins and feet would naturally project below the thigh-pieces, these, too, might be protected, if there were ἐμβάται of leather, the same material of which κρηπῖδες are made. In this way, at one and the same time the shins would be protected and the feet shod with shoes.' Xenophon tells us plainly that the ἐμβάτης of his day was a leather boot covering the shins and so used by cavalrymen in place of greaves. Any assumption of tragic use must be pure theorizing based on evidence from the second century A.D.

κρηπίς. — We have κρηπίς used four times in the sense of shoe in fifth and fourth century literature.

1. Xen. *De re equestr.* 12, 10 (cited under ἐμβάτης) informs us only that the κρηπῖδες were made of leather.

2. Plato Com., *Frg.* Kock, I, p. 612,

<div style="text-align:center">

τίθημι κοττάβεια σφῶν ἐγὼ
 τασδί τε τὰς κρηπῖδας ἃς αὕτη φορεῖ,
καὶ τὸν κότυλον τὸν σόν.

</div>

Here we find a woman the wearer.

3. Hippocr. *De Artic.* 828 C, [χρὴ] ὑποδημάτιον δὲ ποιέεσθαι μολύ-
βδινον ἔξωθεν τῆς ἐπιδέσιος ἐπιδεδεμένον, οἷον αἱ Χῖαι κρηπῖδες ῥυθμὸν
εἶχον. From this we learn that there were several styles, one of which
was called the Chian.

4. Theophr. *Char.* 2, καὶ συνωνούμενος δὲ κρηπῖδας, τὸν πόδα φῆσαι
εἶναι εὐρυθμότερον τοῦ ὑποδήματος.

None of these passages gives us any definite conception of the κρηπίς,
but its military use is as probable as any, especially as at the beginning
of the third century we find Theocritus singing, *Id.* 15, 6,

<div align="center">παντᾷ κρηπῖδες, παντᾷ χλαμυδηφόροι ἄνδρες.</div>

But the common classical meaning of the word is 'foundation,' in which
sense, only, Aeschylus, Euripides, and Sophocles use it. Therefore,
the evidence seems to show that when used for the name of a shoe,
κρηπίς, in literature of the classical period, whatever its more exact
signification, did not mean tragic shoe.

ὀκρίβας. — The true meaning of this word is, and always has been, a
puzzle. The various meanings which have been given to it are well
summed up by Hesychius who, after giving the definitions ἐμβάτης and
ὄχημα ἡνιόχου, proceeds, οἱ μὲν ὄνον φασίν, οἱ δὲ ἄγριον κριόν, ἄλλοι
κλίμακα. κυρίως δὲ τὸ λογεῖον, ἐφ' οὗ οἱ τραγῳδοὶ ἠγωνίζοντο· τινὲς
δὲ κιλλίβας τρισκελής, ἐφ' οὗ ἵσταντο οἱ ὑποκριταὶ καὶ τὰ ἐκ μετεώρου
λέγουσιν. The general drift of these words 'ass = easel,' 'ladder,'
'speaking-place,' 'three-legged stool,' tends towards the definition of a
platform or stand of some light, temporary sort.

Our *locus classicus*, the only occurrence of the word in literature of
this period, is the well-known, much-discussed passage from the *Sympo-
sium* of Plato, 194 B, Ἐπιλήσμων μέντ' ἂν εἴην, ὦ Ἀγάθων, εἰπεῖν τὸν
Σωκράτη, εἰ ἰδὼν τὴν σὴν ἀνδρείαν . . . ἀναβαίνοντος ἐπὶ τὸν ὀκρίβαντα
μετὰ τῶν ὑποκριτῶν καὶ βλέψαντος ἐναντία τοσούτῳ θεάτρῳ, μέλλοντος
ἐπιδείξεσθαι σαυτοῦ λόγους, καὶ οὐδ' ὁπωστιοῦν ἐκπλαγέντος, νῦν οἰη-
θείην σε θορυβηθήσεσθαι ἕνεκα ἡμῶν ὀλίγων ἀνθρώπων. The use of the
word here in the singular prohibits the idea of mounting upon high-soled
shoes. The lexicographers, as we shall find in considering the imperial
and later periods of Greek history, entirely misunderstood this passage,
and it is only gradually that the archaeologists of to-day have been
arriving at some idea of its true meaning.

The passage refers to the Proagon,[1] during which the ἐπίδειξις of poet and actors took place,[2] probably in the Odeon, and the ὀκρίβας must be the βῆμα or some temporary stand erected that the poets and actors at the coming great festival might stand on it and be viewed by the crowds. Therefore, while the ὀκρίβας probably denotes some elevation, it cannot be the high-soled buskin.

None of the words which have been discussed applies to a high tragic boot of the classical period; nor yet do any of the other very numerous words for shoe or sandal occur in any connection to justify their interpretation as tragic high-soled boot.[3] These need not be taken up, but there is one style of sandal distinguished from the rest by the addition of Τυρρηνικά, which deserves special mention.

σανδάλια Τυρρηνικά. — No ancient authority ever thought of these as tragic foot-gear; but Meineke, *Com. Graec.* II, Part 1, p. 91, in his remarks on the fragment from Cratinus— Σανδάλια Τυρρηνικά —quoted by Pollux, 7, 86, makes Hesychius his authority for describing them as having κάττυμά τι ὑψηλόν, or high sole, and so "*simillimi . . . cothurnis.*" Even if they had a marked resemblance to the *cothurnus*, these σανδάλια were not put to the same use, but were given to majestic figures like the Athena Parthenos. Poll. 7, 92, σανδάλιον γὰρ ἦν, ὑπέδησε δ᾽ αὐτὸ Φειδίας τὴν Ἀθηνᾶν.

Inasmuch as we have found no word in literature of the classical period for a high-soled tragic boot, we shall next take up the dramas themselves and see how far they support the view that the boot worn by tragic actors did not have an elevated sole.

THE DRAMAS

In the study of these it must be remembered that an ordinary shoe would present no impediment to the action, but that a boot with a sole from four to ten inches high would present great difficulties to intense action. Arnold (Baumeister's *Denkm.* III, pp. 1575–6) maintains that the dramatic action was limited to motions of the head, arms, fingers,

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[1] Rohde, *Rhein. Mus.* XXXVIII, p. 251 ff.

[2] Cf. *Vita Eur.* p. 3, 11 (Schwartz).

[3] A full treatment of these other words may be found in the article by A. A. Bryant on *Greek Shoes in the Classical Period, Harv. Stud. Class. Phil.* X, p. 57 ff., to which I am greatly indebted for help in the preceding pages of this article.

and the swaying of the body; but Bethe (*Proleg. z. Gesch. d. Theat.*, p. 324) with greater justice, in my opinion, cites the Parodos of the *Seven Against Thebes* and scenes from the *Suppliants* of Aeschylus as places where intense feeling had free play, while he mentions the close of the *Choephori* and the earlier part of the *Eumenides* as scenes demanding intense, lively action. I must part company with Bethe, however, when, assuming the use of the high-soled buskin as unquestionable, he tries to reconcile the violent action of the dramas with the difficulties presented by such foot-gear. He speaks of the activity shown in the use of similar shoes by the Venetian women of to-day, on which they walk easily, kneel, and rise without trouble. Even though these are not so high as the shoes he thinks the actors wore, nevertheless he believes the same agility was possible on the high soles of the Greeks, because the action is required by the dramas.

While this may hold true for soles of three inches and under, it hardly seems possible with the ten-inch sole which modern authorities give to tragedy in the classical period. For tragedy is full of situations demanding violent action. Besides the more general scenes we have running entrances of characters in every rôle; actors climb steps or ladders, fall and rise readily, creep on the ground, lie upon it for long periods at a time, kneel in supplication, and even remove their shoes. We now take these up more in detail.

General Scenes.— Besides the scenes from the *Septem*, the *Suppliants*, the *Choephori*, and the *Eumenides* already mentioned, we find many scenes, particularly in Euripides, where the words of the dramas themselves explain the stage business and require intense action. Twice in the *Prometheus Bound* of Aeschylus we are told of the leapings of Io, which, if not literally carried out, must have been indicated by some violent action, as is suggested by v. 599 ff.,

> σκιρτημάτων δὲ νήστισιν αἰκίαις
> λαβρόσυτος ἦλθον,

and she rushes out with the cry, 882–886,

> τροχοδινεῖται δ' ὄμμαθ' ἑλίγδην,
> ἔξω δὲ δρόμου φέρομαι λύσσης
> πνεύματι μάργῳ, γλώσσης ἀκρατής·
> θολεροὶ δὲ λόγοι παίουσ' εἰκῇ
> στυγνῆς πρὸς κύμασιν ἄτης.

Violent scenes in Euripides are numerous. Orestes describes his own actions, Eur. *Or.* 278,

> ποῖ ποῖ ποθ᾽ ἡλάμεσθα δεμνίων ἄπο;

The two old men, Cadmus and Teiresias, act in most undignified fashion in *Bacchae*, 184–5,

> ποῖ δεῖ χορεύειν, ποῖ καθιστάναι πόδα
> καὶ κρᾶτα σεῖσαι πολιόν;

cf. 190, 195 ff., also the words of Pentheus, 248–251,

> ἀτὰρ τόδ᾽ ἄλλο θαῦμα, τὸν τερασκόπον
> ἐν ποικίλαισι νεβρίσι Τειρεσίαν ὁρῶ
> πατέρα τε μητρὸς τῆς ἐμῆς, πολὺν γέλων,
> νάρθηκι βακχεύοντ᾽·

and the reply of Teiresias refusing to stop his dance in honor of Dionysus, 322–324,

> ἐγὼ μὲν οὖν καὶ Κάδμος, ὃν σὺ διαγελᾷς,
> κισσῷ τ᾽ ἐρεψόμεθα καὶ χορεύσομεν,
> πολιὰ ξυνωρίς, ἀλλ᾽ ὅμως χορευτέον.

Helen rushes to the altar pursued by Menelaus and throws herself upon it, as we see from *Hel.* 546–556; and there are a number of other scenes — *Heracl.* 69–79, cf. 128; *Hec.* 1056–1059; *Or.* 211–235 and 1554 ff.; *Phoen.* 315–16; *I. A.* 314–320, 631, and *Ion*, 1250 ff. — in which activity of motion is spoken of. No one, I think, would expect Euadne to wear high-soled boots when she leaps into the funeral pyre, *Suppl.* 1069–1071.

Hurried Entrances. 1. *Of Messengers.* — Running is demanded in the stage action by the words of the chorus, Aesch. *Pers.* 246–248,

> ἀλλ᾽ ἐμοὶ δοκεῖν τάχ᾽ εἴσῃ πάντα ναμερτῆ λόγον.
> τοῦδε γὰρ δράμημα φωτὸς Περσικὸν πρέπει μαθεῖν,
> καὶ φέρει σαφές τι πρᾶγος ἐσθλὸν ἢ κακὸν κλύειν,

and messengers enter in haste (σπουδῇ), if not running, Aesch. *Sept.* 39 (cf. 54), 371; Eur. *Hipp.* 1151–2; *Ion*, 1109–10; *Med.* 1118–1120; *Tro.* 232. Cf. also the parody in Ar. *Av.* 1121 ff.

2. *Of Heroic Characters.* — Running was not confined to the lesser rôles. Heroes, too, enter on the run. Eur. *Or.* 725–6,

> ἀλλ' εἰσορῶ γὰρ τόνδε φίλτατον βροτῶν
> Πυλάδην δρόμῳ στείχοντα Φωκέων ἄπο,

Eur. *Tro.* 306–7,

> οὐκ ἔστιν, οὐ πιμπρᾶσιν, ἀλλὰ παῖς ἐμὴ
> μαινὰς θοάζει δεῦρο Κασάνδρα δρόμῳ.

Entrances at great speed (ἀρτίπους) are made according to Soph. *El.* 871–2 (cf. 934–5); *Oed. Col.* 885–890; *Trach.* 58 ff. Eur. *Bacch.* 1230 (cf. 1168); *Or.* 1505, 1550; and in great haste (σπουδῇ) according to Aesch. *Sept.* 374. Soph. *Ai.* 1223–4; *Phil.* 1223. Eur. *Andr.* 545–6, 824–840, 880; *Bacch.* 212, 642 (cf. 647); *Hec.* 216–17; *Heracl.* 118; *Hipp.* 902–3; *Ion*, 1253; *Rhes.* 85.

Climbing Scenes. — Twice heroic characters climb steps. Although the actual process is not described, we know that Danaus has climbed up the steps of the great altar from his words, Aesch. *Suppl.* 713–14,

> ἱκεταδόκου γὰρ τῆσδ' ἀπὸ σκοπῆς ὁρῶ
> τὸ πλοῖον. εὔσημον γὰρ οὔ με λανθάνει·

And another scene from the *Phoenissae* of Euripides, 103–106, shows us an old pedagogue helping Antigone up a ladder or flight of steps to the top of her house,

> AN. ὄρεγέ νυν ὄρεγε γεραιὰν νέᾳ
> χεῖρ', ἀπὸ κλιμάκων ποδὸς
> ἴχνος ἐπαντέλλων.
> ΠΑΙ. ἰδοὺ ξύναψον, παρθέν'· εἰς καιρὸν δ' ἔβης.

It is hard to imagine these scenes being carried out with the actors raised upon soles five to ten inches high.

Falling and Rising Scenes. — The most violent fall is taken by Iolaus in Eur. *Heracl.* 75–79,

> IO. ἴδετε τὸν γέροντ' ἀμαλὸν ἐπὶ πέδῳ
> χύμενον· ὦ τάλας.
> XO. πρὸς τοῦ ποτ' ἐν γῇ πτῶμα δύστηνον πίτνεις;
> . . . .
> IO. ὅδ', ὦ ξένοι, με σοὺς ἀτιμάζων θεοὺς
> ἕλκει βιαίως Ζηνὸς ἐκ προβωμίων.

And we are told that his legs had been knocked from under him in the words of the chorus, 127 ff.,

> βίᾳ νιν οὗτος τῆσδ' ἀπ' ἐσχάρας ἄγειν
> ζητῶν βοὴν ἔστησε κἄσφηλεν γόνυ
> γέροντος, ὥστε μ' ἐκβαλεῖν οἴκτῳ δάκρυ.

He falls again when Macaria goes off, but this time from faintness not from violence, v. 602. He is bidden to rise by a servant in 635 and evidently raises himself up. Similar scenes, where the actors swoon upon hearing bad news, are to be found in Eur. *Andr.* 1076–1078 ; *Hec.* 438–502 ; *Tro.* 462–465 ; and Philoctetes, 820–893, is put to sleep by the pain from his poisoned foot. Having once fallen the persons are all obliged to rise again, and they invariably do it unassisted when addressed by the words ἔπαιρε σαυτὸν. The falling would be easy enough, but rising upon pedestals of leather or wood, such as the high soles must have been, would have been extremely awkward.

Lying Scenes. — Many of the scenes included under the preceding heading involve lying on the ground for some time (e. g. Soph. *Phil.* 820–879), but two of the plays of Euripides begin with actors lying prostrate on the ground. It is Adrastus who lies with his head hid in his mantle in *Suppl.* 1–112, and we are first made aware of this by the words of Aethra, 20–22,

> κοινὸν δὲ φόρτον ταῖσδ' ἔχων χρείας ἐμῆς
> Ἄδραστος ὄμμα δάκρυσιν τέγγων ὅδε
> κεῖται.

He continues in this posture until v. 112 when the words of Theseus compel him to stand up. The *Troades* opens with Hecabe lying before the palace. She remains prostrate for 120 verses. Then she, too, rises. It is not necessary to suppose that the high soles could be seen by the spectators while the actors were lying on the stage, but, if they were visible, as is very possible, they must have made a ridiculous sight. At a much later date we find Lucian (*Somn.* 26) ridiculing the momentary exposure of such a boot. Another similar scene occurs towards the end of *Hercules Furens*, vv. 1159–1402, in which Heracles seats himself on the ground with his mantle over his head mourning for the murders he has committed. He as well as the others rises from this position, thereby showing that it was a comparatively easy thing to rise from any position

with the shoes that were worn. It is difficult to conceive of this with high soles, ten inches high.

Creeping Scenes. — Furthermore, two characters creep about before the spectators: the aged priestess, Aesch. *Eum.* 34 ff.,

> ἦ δεινὰ λέξαι, δεινὰ δ' ὀφθαλμοῖς δρακεῖν
> πάλιν μ' ἔπεμψεν ἐκ δόμων τῶν Λοξίου,
> ὡς μήτε σωκεῖν μήτε μ' ἀκταίνειν βάσιν·
> τρέχω δὲ χερσίν, οὐ ποδωκείᾳ σκελῶν·
> δείσασα γὰρ γραῦς οὐδέν, ἀντίπαις μὲν οὖν,

and Polymestor, Eur. *Hec.* 1056–1059,

> ὤμοι ἐγώ, πᾶ βῶ,
> πᾶ στῶ, πᾶ κέλσω;
> τετράποδος βάσιν θηρὸς ὀρεστέρου
> τιθέμενος ἐπὶ χεῖρα κατ' ἴχνος;

In these any high-soled buskin worn must have been in full sight of the audience — a strange thing to believe of the Greek drama. Thus, another difficult posture is presented to us from which apparently the actors raised themselves to their feet without trouble.

Scenes of Supplication. — Perhaps the most difficult feat of all to accomplish with a ten-inch sole would be the act of supplication, in which one actor sank upon his knee and stroked the chin and grasped the knee of another. Not to speak of the height to which the chin of the latter would be raised by high-soled buskins, the mere act of kneeling on the ground with one foot on a leather pedestal of five to ten inches must have been in the highest degree uncomfortable and at the same time awkward to look at. And yet actors assumed this position many times.

Soph. *Phil.* 485–6 (Philoctetes to Neoptolemus),

> πείσθητι· προσπίτνω σε γόνασι, καίπερ ὢν
> ἀκράτωρ ὁ τλήμων, χωλός.

Eur. *Andr.* 572–575, cf. 717 ff. (Andromache to Peleus),

> ἀλλ' ἀντιάζω σ', ὦ γέρον, τῶν σῶν πάρος
> πίτνουσα γονάτων, χειρὶ δ' οὐκ ἔξεστί μοι
> τῆς σῆς λαβέσθαι φιλτάτης γενειάδος,
> ῥῦσαί με πρὸς θεῶν.

Ibid. 892–895 (Hermione to Orestes),

> Ἀγαμέμνονος παῖ, πρός σε τῶνδε γουνάτων
> οἴκτιρον ἡμᾶς ὧν ἐπισκοπεῖς τύχας,
> πράσσοντας οὐκ εὖ. στεμμάτων δ' οὐκ ἥσσονας
> σοῖς προστίθημι γόνασιν ὠλένας ἐμάς.

Hipp. 325–6, cf. 333 (Nurse to Phaedra),

> ΦΑΙ. τί δρᾷς; βιάζει χειρὸς ἐξαρτωμένη;
> ΤΡ. καὶ σῶν γε γονάτων, κοὐ μεθήσομαί ποτε.

Other instances abound: Soph. *Oed. Col.* 1754. Eur. *Hec.* 275 (cf. 273, 286), 339 (cf. 344), 752–3 (cf. 787, 812); *Hel.* 894, 1236–7; *Her. Fur.* 1208; *Hipp.* 605–607; *I. A.* 900 ff. (cf. 908–910), 1215–1217; *Med.* 324, 709–10; *Or.* 382, 1507; *Phoen.* 923; *Suppl.* 165; *Tro.* 1042.

Hurried Exits. — We have seen that actors of the chief as well as of the less important rôles enter in great haste, and, during the scene, dance, run about, fall, creep, and rise, all in the most natural way. Now we shall see that in making their exits they must often have shown speed and violent action. Such must have been true of Jocasta if she acted as the words of the chorus, Soph. *Oed. Tyr.* 1073–4, lead us to imagine,

> τί ποτε βέβηκεν, Οἰδίπους, ὑπ' ἀγρίας
> ᾄξασα λύπης ἡ γυνή;

Nor can we believe in a quiet exit of Orestes in Aesch. *Choeph.* 1061–2. The text gives us no cues on this point, but his own words show the pitch to which his feelings have risen. The Dread Goddesses are all about him to his fevered imagination. He is unable to escape them, and bursts off the scene with the cry, 1061–2,

> ὑμεῖς μὲν οὐχ ὁρᾶτε τάσδ', ἐγὼ δ' ὁρῶ·
> ἐλαύνομαι δὲ κοὐκέτ' ἂν μείναιμ' ἐγώ.

In other plays, likewise, the words with which the characters leave the scene betray violent emotions and leave little doubt of intense action in the exit. Such are Eur. *Elect.* 216 ff.; *Hipp.* 599–600; *Phoen.* 1280 ff. In fact, it is impossible to escape the conviction that the Greek drama carried out in action the emotions expressed so often in words, un-

hampered by any such hindrances as high-soled boots. And not only is the action in general violent, but many situations arise when the use of a lofty sole is really precluded. The words of Bethe (*Proleg. z. Gesch. d. Theat.*, p. 321) that the actors "richten sich sogar allein ohne Hilfe auf," are borne out by the facts. Adrastus rises unaided after lying before the temple during the recital of over a hundred verses. Hecabe does the same after an equal length of time. So, too, does Iolaus. We have, then, these instances of prominent characters rising to their feet unaided, a thing to be expected with an ordinary shoe, but difficult to imagine if we picture them rising upon a ten-inch pedestal of leather or wood.

There remains to be discussed a very important passage, Aesch. *Ag.* 944–5,

> ἀλλ' εἰ δοκεῖ σοι ταῦθ', ὑπαί τις ἀρβύλας
> λύοι τάχος, πρόδουλον ἔμβασιν ποδός.

Professor Carl Robert (*Hall. Winckelmannspr.* 22, p. 23) was the first person to appreciate the significance of this passage. Agamemnon has just come home from the Trojan war. His base and deceitful wife, Clytemestra, wishes him to make a triumphal entry over a carpet of purple. And he, finally persuaded, says, 'Well, if so it pleaseth thee, let some one quickly undo my shoes, servants to my feet in walking.' The act of taking off the shoes is accomplished between lines 945 and 955. The poet with the greatest art has allowed for that time by introducing the subject of the 'jealousy of the gods,' and by directions for the welfare of his favorite captive, Cassandra. Then, finally, in 956 he turns to enter the palace with the words,

> ἐπεὶ δ' ἀκούειν σοῦ κατέστραμμαι τάδε,
> εἶμ' ἐς δόμων μέλαθρα πορφύρας πατῶν.

Robert brought out the fact that, if Agamemnon had been wearing high-soled boots, their removal would have left him ridiculously shorter than Clytemestra, and that so gross an offence against good taste could not have been permitted.

To me this argument seems perfectly valid. Agamemnon gives directions for the removal of his travelling shoes, waits while it is being done, then enters the door near which he is standing. If it is granted that his shoes were removed, we cannot escape the conclusion that they

were ordinary, not high-soled, shoes ; for, otherwise, not only would he be ridiculously cut off in stature, but his long chiton ($\chi\iota\tau\grave{\omega}\nu$ $\pi o\delta\acute{\eta}\rho\eta s$), too long now by a number of inches, would trip him or have to be gathered up in most untragic manner.

But some will not be willing to grant that the shoes were taken off. Robert, himself, having become convinced from a little painting found in Herculaneum of the use of an eight or ten-inch sole in the fifth century, took back his earlier assertion and formulated the hypothesis that there was here only a show of taking off the shoes, and that they were kept on in reality. This hypothesis is not the natural one ; it is rather the result of prejudgment of the point at issue. To one not concerned about the tragic boot the lines would certainly mean that the shoes came off ; and if they came off, they could not have been high-soled.

Our evidence from the drama, therefore, proves that the actors were quick on their feet and easily assumed all manner of difficult postures. They enter and leave in haste, sometimes on the run, they dance and leap, creep on hands and knees, kneel in supplication, and rise to their feet unaided. So much our evidence tells us. It is, therefore, opposed to the use of any boot with a high sole. But it is not conclusive evidence and can be valuable only as it corroborates our evidence from other sources.

Evidence from Contemporary Art

Perhaps the most convincing proof that tragic actors did not wear high-soled shoes is the fact that such shoes are not to be found in any of the monuments of the period and are notably lacking in the one piece of sculpture which renders most faithfully the dress of a tragic actor of this period. This is the Piraeus Relief.[1]

Piraeus Relief. — This was erected as a choregic monument in the Piraeus at the close of the fifth century or the beginning of the fourth.[2] It stood for a long time a striking example of sculpture of the later Pheidian school. At a subsequent date another hand, unwilling that the figures on it should be nameless, scratched two names (perhaps more)

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[1] Published by Robert, *Ath. Mitt.* VII, pl. XIV, p. 389.

[2] So dated by Robert, *l. c.*, p. 392; and by Friedrichs-Wolters, *Die Gipsabgüsse Antiker Bildwerke*, No. 1135, Berlin, 1885.

on the base — one, ΔΙΟΝΥΣΟΣ, the other, almost obliterated, probably ΠΑΡΑΛΙΑ.[1] In the troublous times that followed it was overturned and found a resting place in the harbor. It was recovered in our own times, little damaged by its long stay beneath the waves, and was brought to its present home in the Museum of the Piraeus. The Berlin Museum possesses a cast (No. 1135).

In conception the relief is closely analogous to the commonly depicted "Death-Feasts," [2] though used here for a choregic votive offering. At the right, on a couch, reclines a young man, who holds upraised in his right hand a drinking-horn and in his left, close by his side, a shallow bowl. At his feet sits a maiden, who wears the nebris in addition to her chiton.[3] The left-hand half of the relief is taken up by the standing figures of three young men, who are readily identified as actors by their costumes and the masks which two of them carry. Two, also, carry mirrors.[4] Evidently, the scene is one of preparation for the play. The actor who has no mask must have been represented with it already on. His head has been badly mutilated, but the position of the mirror, which he is holding up in his left hand, puts it beyond a doubt that he was represented taking a final look at the way his mask set. The other two are not so nearly ready and have the masks still in their hands.

As Agamemnon, last of all, before seizing his spears (*Il.* 11, 41),

> κρατὶ δ᾽ ἐπ᾽ ἀμφίφαλον κυνέην θέτο τετραφάληρον
> ἵππουριν· δεινὸν δὲ λόφος καθύπερθεν ἔνευεν,

so, I feel certain, the actors put on their masks last of all. In dressing, anything that is going to embarrass our heads we leave to the end. It would be both unnatural and inconvenient for an actor to put on his

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[1] Robert, *l. c.*, p. 391, reads ΠΑΙΔΗΑ, but Schuchhardt, *Ath. Mitt.* XIII, p. 221, finds on the stone Γ Ρ / ΛΙΑ ⸗ ΠΑΡΑΛΙΑ.

[2] Furtwängler, *Collection Sabouroff*, I, pls. 30–33, Berlin, 1883–1887.

[3] For attempts at identifying these two figures see Robert and Friedrichs-Wolters, *l. c.*; also Robert, *Scenisches, Hermes*, XXII, p. 336; and finally Schuchhardt, *Paralia, Ath. Mitt.* XIII, p. 221 ff.

[4] These were first regarded by Robert and, following him, Friedrichs as tambourines (tympana), but were finally identified by Robert, *Scenisches, Hermes*, XXII, 336, as mirrors, by which the actors are adjusting their masks.

mask before he had properly adjusted his chiton and drawn on his shoes. Therefore, since one actor undoubtedly has his mask on and the other two have theirs in their hands and the mirrors with which to adjust them, we may feel certain that in this relief we see the actors as they were just before appearing in the orchestra for the performance.

Nothing seems lacking in the costume. The long chiton (χιτὼν ποδήρης) is the same as that which persisted in tragedy down through imperial times. The broad girdle confines it high up on the waist. Close-fitting sleeves reach to the wrist and closed shoes cover their feet. But how different these shoes are from the high-soled buskin modern authorities would attribute to this period! They are only partially concealed and we can easily tell their type. They are closed boots, but *without any elevation*. As I have shown above, this is almost the last minute before the actors enter the orchestra. Therefore, if they had been accustomed to use high-soled boots, they would have been on by this time. But not only are the high-soled shoes missing, they are even replaced by other shoes with soles of ordinary thickness.

The evidence to be derived from this relief is of the most positive nature. The style is such that the date is indisputable; the costumes of the three figures are so complete that their identity as actors is fixed; finally, they have their stage-shoes already on and these are *not elevated*.

In the light of the knowledge derived from the Piraeus relief, we may consider another monument of this period, an amphora in the Museum of Naples, which has acquired from the paintings upon it the name Satyr-Vase. It is one of the two earliest pieces of evidence extant for the costume of tragedy, the other being a crater, in the Berlin Museum, decorated with the scene of Andromeda's rescue from the sea-monster by Perseus.

Satyr-Vase[1] and *Andromeda Crater*.[2] — The usefulness of the satyr-vase as evidence is made plain by comparing the costumes shown upon it with the costumes upon the crater, where a purely tragic scene is

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[1] Published, *Mon. Ined.* III, 31; Baumeister's *Denkm.*, pl. V; Schreiber, *Culturhist. Bilderatlas*, pl. 3; Wieseler, *Theatergebäude und Denkmäler des Bühnenwesens*, pl. VI, 2, Göttingen, 1851.

[2] Published, *Jahrb.*, 1896, p. 292 and pl. 2; Englemann, *Archaeol. Studien z. d. Tragikern*, fig. 20, Berlin, 1900.

represented. By such a comparison Bethe[1] proves that the costume of the chief actors of a satyr play was identical with the costume worn by actors of tragedy. Like costume bespeaks a like origin, and we find Aristotle saying of tragedy, *Poetics*, 1449 a, 20, διὰ τὸ ἐκ σατυρικοῦ μεταβαλεῖν.

If Bethe's proof holds, we can look to the Satyr-Vase for evidence on the shoes of the actors as well as the rest of the costume. The painter of the crater has taken advantage of his liberties and has represented Andromeda with bare feet. The actors on the Satyr-Vase, however, wear closed shoes without any high sole. As Müller,[2] Navarre,[3] and Haigh[4] say, the high sole may be left off for the sake of activity, to which the shorter chitons might point; but Bethe argues that it was left out for artistic reasons, or because it was put on just before entering the scene.[5] Vase-painting does take such liberties with the facts that we cannot lay much stress on evidence derived from it. Where it corroborates other trustworthy evidence, however, there it is valuable. The Satyr-Vase makes more certain the results obtained from a study of the Piraeus relief.

Furthermore, a careful search through the whole range of sculpture, vase-paintings, and terra-cotta figurines fails to reveal anything in the nature of a high-soled shoe. The grave-reliefs[6] often show very thick sandals; such seem to have been common foot-wear. But none can exceed a height of two inches and none have any connection with tragedy. Not even the South Italian vases, which often have very faithful reproductions of scenes from tragedy, show high-soled buskins.[7] But, as most of the scenes are treated more as living stories than as stage representations, we cannot lay much emphasis on this fact. A striking contrast to later times when the use of the high sole is a certain fact is to be remarked in the absence of this part of the dress as a

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[1] *Jahrb.*, 1896, p. 292 ff. Cf. Arnold in Baumeister's *Denkm*. III, 1568–1571. Also Daremberg et Saglio, *Dict. d. Ant.* s. v. *Cothurnus*. Cf. Hor. *Sat.* 1, 5, 64.

[2] *Lehrb. d. griech. Bühnenalt.*, p. 242, Freiburg i. B., 1886.

[3] *Dionysos*, p. 185, Paris, 1895.

[4] *The Attic Theatre*[2], p. 287, Oxford, 1898.

[5] *Proleg. z. Gesch. d. Theat.*, p. 37, note 13.

[6] Conze, *Attische Grabreliefs*, plates, Berlin, 1893–1904.

[7] Englemann, *Archaeol. Studien z. d. Tragikern*. Huddilston, *Greek Tragedy in the Light of Vase Paintings*, London, 1898.

symbol of tragedy. Whereas in the Imperial period we find the mask and the buskin together the symbols of tragedy, in this period the mask stands alone. Therefore, we find art joining with literature and denying emphatically for this period the existence of a high-soled tragic shoe.

Our study of the evidence on the use of the high-soled buskin cannot be confined to the classical period alone, for almost all the evidence in favor of the use is obtained from later literature and art. These must be examined and their bearing on the use of the high-soled boot in the classical period ascertained. While this article does not attempt to settle the time when the high-soled shoe was introduced into tragedy, the argument against the classic use of the high-soled boot may be materially strengthened by showing that such a boot was not used for two centuries after the classical period, or, in other words, was not used during the Hellenistic period.

EVIDENCE FROM HELLENISTIC LITERATURE

Dierks[1] discovers evidence for the use of the boot in this period in the description of the festival procession of Ptolemy Philadelphus, given by Callixenus and quoted by Athenaeus, 5, 198 a. We find that the procession was made up of fantastic personages representing the various chief divinities, each god in turn having a little company of his own followers to represent different features of his worship. In this manner Dionysus was accompanied by Sileni, Satyrs, Winged Victories, boys clad in purple, and others. Then we read (198 a), μεθ' οὓς Σιληνοὶ δύο ἐν πορφυραῖς χλαμύσι καὶ κρηπῖσι λευκαῖς. εἶχε δ' αὐτῶν ὁ μὲν πέτασον καὶ κηρύκειον χρυσοῦν, ὁ δὲ σάλπιγγα. μέσος δὲ τούτων ἐβάδιζεν ἀνὴρ μείζων, τετράπηχυς, ἐν τραγικῇ διαθέσει καὶ προσώπῳ, φέρων χρυσοῦν Ἀμαλθείας κέρας · ὃς προσηγορεύετο Ἐνιαυτός. 'Between the two Sileni walked the *Year*, taller than they, a six-footer, in tragic make-up and wearing a mask.' Dierks argues, " Si ad talem magnitudinem 'corporis respicimus, summa veritatis specie conicere licet illum cothurnos peraltos gestasse." He takes exception to Maass[2] who wishes to explain this passage by some other means than the *cothurnus*, quoting Apollodorus 2, 4, 9, ἦν δὲ (Heracles) καὶ θεωρηθεὶς φοβερὸς

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[1] *De Trag. Graec. Hab. Scaen.*, p. 10.

[2] *Annali d. Instit.*, 1881, p. 115, note 1.

ὅτι παῖς Διὸς ἦν. τετραπηχυαῖον μὲν εἶχε τὸ σῶμα. "Sed fieri non potest," continues Dierks, "ut hi loci inter se comparentur.   Nam Apoll. de Herculis magnitudine loquitur, quomodo in cultu ficta sit, Callix. vero de histrione quodam.   Ratio autem inter illos locos haec est : Graeci quia heroes ingenti corporis magnitudine fuisse existimabant, histriones cum cothurnis prodeuntes fecerunt, ut speciei illorum adaequarentur.   Quam fuisse causam usus cothurnorum, expressis verbis indicatur a Philostr. (*Vita Apoll.* VI, 11) ὀκρίβαντος δὲ τοὺς ὑποκριτὰς ἀνεβίβασεν ὡς ἴσα ἐκείνοις (ἥρωσι) βαίνοιεν."

The firmly rooted conviction that high-soled boots were part of the τραγικὴ διάθεσις of this period leads Dierks into the error of forgetting that there were many men in those days, as in the present, who needed no elevated shoes to make them six-footers.   Such men were noticed then as now.[1]   To carry the horn of Amalthea such a man might well have been picked, a man conspicuous for his height.   We may feel that a grander spectacle would have been secured by mounting him and the gods as well on high-soled boots ; but, so far as we know, that was not done here.   And, even if it could be proved for this procession, it would not be convincing evidence for the usage of the drama.   Because fantastic personages appear in our processions ten feet tall, it is no proof that they would appear thus represented on the stage.

Furthermore, those of the later words for high-soled tragic boot which have been preserved to us in the scanty literature of this period show no evidence of having yet been perverted from their classical meaning ; ἀρβύλη is still the poetical word for shoe.   Theocritus, *Id.* 7, 26,

πᾶσα λίθος πταίοισά ποτ' ἀρβυλίδεσσιν ἀείδει.

Lycophron, *Alex.* 837–839,

ἀντὶ θηλείας δ' ἔβη
τὸν χρυσόπατρον μόρφον ἁρπάσας γνάθοις,
τὸν ἡπατουργὸν ἄρσεν' ἀρβυλόπτερον.

And κόθορνος again occurs with κροκωτός (if the fragment be correctly restored) as a woman's boot covering the shins.   Herodas, *Mimiamb.* 8, 69,

. . . . . ἀμφίκν[ημας] . . .
. . . . κο]θόρνου[ς . . . . . .

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[1] Cf. Ar. *Ran.* 1014; *Vesp.* 553.

-κροκωτ- occurring six lines before. The κρηπίς is worn by soldiers, Theocr. *Id.* 15, 6 (see p. 133), and by Sileni in a festival procession, Callixenus ap. Athen. 5, 198 a (see p. 146). This can be no tragic high-soled boot. The κρηπῖδες are mentioned here because of their unusual color, white, as are the chlamydes because they are purple.

Nor do any of the side-lights on the drama of this period favor the use of a high-soled shoe. Just as prize-fighters of the present day take to the stage to exhibit their skill, so we can believe the professional boxers of Greece did. For such a man set up a dedicatory inscription in this period to commemorate his many victories in the revived plays of Euripides.[1] As was to be expected, we find the titles of the plays in which he performed to have been those of the most violent plays of Euripides, those demanding a great show of excitement and liveliness. Are we to suppose that he wore high-soled boots and so hindered, if not absolutely prohibiting, a display of his superior muscle?

Thus the Hellenistic period seems not to have made use of the high-soled buskin any more than the classical period. This we shall find corroborated by later copies of art of this period. None of the actual remains of the art of this period have any bearing on the question of the high-soled tragic shoe.

Thus far we have examined literature and art down to the year 150 B.C. and have found no trace of such a boot in art, no name for it, no mention of it in literature. But in art shortly after this we find the high-soled shoe and in literature of the next century we find it mentioned. What evidence for the classic use of the shoe does this later literature give us?

EVIDENCE FROM LITERATURE OF THE IMPERIAL AND LATER PERIODS

The high-soled boot of these times was called by six different names, as I have said before (p. 130). These I shall take up in alphabetical order.

ἀρβύλη. — Suidas attributes to Aeschylus the introduction of ἀρβύλαι as part of the costume of tragedy and translates by the better known term ἐμβάται, s. v. Αἰσχύλος· οὗτος πρῶτος εὗρε προσωπεῖα δεινὰ καὶ χρώμασι κεχρισμένα ἔχειν τοὺς τραγικούς, καὶ ταῖς ἀρβύλαις τοῖς καλου-

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[1] Herzog, *Ein Athlet als Schauspieler, Philol.* LX, p. 440 ff.

μένοις ἐμβάταις κεχρῆσθαι.   Because of its frequent use in the dramas Suidas must have committed the error of considering it the technical word in classical times for the ἐμβάται of his own time.   For never again is it found with this signification, but it is found with other definite meanings: Poll. 7, 86, ἦν δέ τι ὑπόδημα καὶ ἀρβύλη, εὐτελὲς τὴν ἐργασίαν.   Photius, *Biblioth.* (Bekker), p. 2 b, 18, [ἀνεγνώθη] ὅτι τὰ σανδάλια, φησί, νυνὶ λεγόμενα ἀρβύλας ἔλεγον οἱ παλαιοί.   Hesychius, s. v. ἀρβύλαι.   εἶδος ὑποδημάτων.   *Etym. Magn.* s. v. ἀρβύλη· παρὰ τὸ ἁρμόζεσθαι τοῖς ποσὶν, ἁρμύλη, καὶ ἀρβύλη.   Ἔστιν δὲ εἶδος ὑπο-δήματος περισσῶς εἰργασμένον.   *Anthol. Pal.* 16, 306–308, in three different epigrams describes the same statue of Anacreon wearing ἀρβυλίδες, βλαύτια, σάνδαλα.   By this array of authorities who would undoubtedly have mentioned the tragic use of the ἀρβύλη, if there had been one, the statement of Suidas is discredited, and we may be per-mitted to conclude that ἀρβύλη was never the name for the high-soled tragic boot.

ἐμβάς and **ἐμβάτης.** — The similarity in spelling of these two words has caused endless confusion and contradiction among the writers of late Greek literature.   The weight of the evidence, however, seems to fall to ἐμβάτης as the proper name for the high-soled tragic boot.   The ἐμβάς is so called by Epictetus, ap. Arrian, *Diatrib. Epictet.* 1, 29, 41–43, ἔσται χρόνος τάχα, ἐν ᾧ οἱ τραγῳδοὶ οἰήσονται ἑαυτοὺς εἶναι προσωπεῖα καὶ ἐμβάδας καὶ τὸ σύρμα. . . . ἂν ἀφέλῃ τις αὐτοῦ καὶ τάς ἐμβάδας καὶ τὸ προσωπεῖον καὶ ἐν εἰδώλῳ αὐτὸν προσαγάγῃ, ἀπώ-λετο ὁ τραγῳδὸς ἢ μένει· ἂν φωνὴν ἔχῃ, μένει· by Lucian, *Scænn.* 26 (according to Reitz), καὶ τῶν ἐμβάδων τὴν ὑπόδεσιν ἀμορφοτάτην καὶ οὐ κατὰ λόγον τοῦ ποδός, where the author is exposing to ridicule the fictions of an actor's make up; by Pollux, 4, 115, καὶ τὰ ὑποδήματα κόθορνοι μὲν τὰ τραγικὰ καὶ ἐμβάδες, ἐμβάται δὲ τὰ κωμικά, and 7, 85, ἐμβάδες· εὐτελὲς μὲν τὸ ὑπόδημα, Θράκιον δὲ τὸ εὕρημα, τὴν δὲ ἰδέαν κοθόρνοις ταπεινοῖς ἔοικεν· in the *Anth. Pal.* 7, 51,

> σὸν δ' οὐ τοῦτον ἐγὼ τίθεμαι τάφον, ἀλλὰ τὰ Βάκχου
> βήματα καὶ σκηνὰς ἐμβάδι πειθομένας.

But the reading here is corrupt; in Bekker's *Anecdota Graeca*, II, p. 746, 18, πρῶτον μὲν ἐπελέγοντο ἄνδρας τοὺς μείζονα φωνὴν ἔχοντας, δεύτερον δὲ βουλόμενοι καὶ τὰ σώματα δεικνύειν ἡρωϊκά, ἐμβάδας ἐφό-ρουν καὶ ἱμάτια ποδήρη.

On the other hand, it is used as in classical literature by Lucian, *Pseud.* 19, σὺ κοσμίως πάνυ χρυσᾶς ἐμβάδας ἔχων καὶ ἐσθῆτα τυρρανικήν · *Rhet. Praec.* 15, ἡ ἐμβὰς Σικυωνία πίλοις τοῖς λευκοῖς ἐπιπρέπουσα · by Synesius, *Epist.* 52, p. 189 c, παρ᾽ οὗ μοι δοκεῖς καὶ πέρυσιν ἐξωνεῖσθαι τὰς ἀνατρήτους ἐμβάδας · *Etym. Magn.* s. v. ἐμβάδες · εἶδος ὑποδήματος · ἀπὸ τοῦ ἐμβαίνειν τοὺς πόδας · Thom. Mag. s. v. ἐμβάδες · ἐμβάδες τὰ κωμικὰ ὑποδήματα, ἐμβάδια δὲ τὰ ἁπλῶς ὑποδήματα · Ammon. *De diff. vocab.* s. v. ἐμβάδες · ἐμβάδες καὶ ἔμβατα διαφέρει. ἐμβάδες μὲν γὰρ, τὰ κωμικὰ ὑποδήματα. ἔμβατα δὲ τὰ τραγικά · Hesych. s. v. ἐμβάς · εἶδος ὑποδήματος · Pseudo-Orpheus, *Argonautica*, 594,

> αὐτὰρ ἐμοὶ μολπῆς γέρας ὤπασε δῖος Ἰήσων
> ἐμβάδα χρυσείῃσι τιταινομένην πτερύγεσσιν ·

Anecd. Graec. (Bekker), p. 249, 25, εἶδος ὑποδήματος. ἴσως παρὰ τὸ ἐμβαίνειν τοὺς πόδας κατωνόμασται. The fact that Lucian twice uses ἐμβάς without any tragic connotation might tend to prove that in the first passage quoted from him (*Somn.* 26) the reading κοθόρνους of Mss. ACT is the correct one. Thus we find ἐμβάς used seven times as tragic boot; but out of that number twice the text is corrupt, twice our authorities, Epictetus and the Anthologia Graeca, are not trustworthy for scenic antiquities, and Pollux is in absolute contradiction to Ammonius, Thomas Magister, Suidas, Libanius, Dio Cassius, Lucian, and the scholiasts, who agree that ἐμβάτης is the correct term for the high-soled boot of tragedy. Pollux calls the ἐμβάτης the shoe of comedy (4, 115), and reiterates this 7, 91, ἐμβάται δὲ ὄνομα τοῖς κωμικοῖς ὑποδήμασιν. He must be incorrect in this and must have interchanged the words ἐμβάς and ἐμβάτης; for the latter is used thirteen times as the boot of tragedy.

Lucian, *Juf. Trag.* 41, implies that three famous actors of earlier centuries[1] wore this ἐμβάτης. But the names are probably used as stock names for tragic actors of any time. His words are, ἀνάγκη δυοῖν θάτερον ἤτοι Πῶλον καὶ Ἀριστόδημον καὶ Σάτυρον ἡγεῖσθαί σε θεοὺς εἶναι τότε ἢ τὰ πρόσωπα τῶν θεῶν αὐτὰ καὶ τοὺς ἐμβάτας καὶ τοὺς ποδήρεις χιτῶνας κτλ. He further shows that it is exactly the high-

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[1] See Völker, *De Graecarum Fabularum Actoribus*, Diss. Hall., 1880.

soled boot which he calls ἐμβάτης, *Necyom.* 16, καταβὰς ἀπὸ τῶν ἐμβατῶν πένης καὶ ταπεινὸς περίεισιν. *Quom. histor. s. conscrib.* 22, ὥστε τὸ πρᾶγμα ἐοικὸς εἶναι τραγῳδῷ τὸν ἕτερον μὲν πόδα ἐπ' ἐμβάτου ὑψηλοῦ βεβηκότι, θάτερον δὲ σανδάλῳ ὑποδεδεμένῳ. *De saltat.* 27, τὴν τραγῳδίαν . . . φοβερὸν θέαμα . . . ἐμβάταις ὑψηλοῖς ἐποχούμενος. The most definite statement of all is the scholiast's definition of ἐμβάται, Luc. *Epist. Saturn.* 19, τὰ ξύλα, ἃ ἐμβάλλουσιν ὑπὸ τοὺς πόδας, ἵνα φανῶσι μακρότεροι. According to the scholium on σκηνή, Soph. *Ai.* 3, they are a part of the theatrical σκευή: σκηνή. ἡ πρόσκαιρος κατοικία, ἣν Ὅμηρος κλισίην λέγει, καὶ ἡ τῶν δραματουργῶν σκευή, ἤ γουν τὰ προσωπεῖα, οἱ ἐμβάται, καὶ αἱ τοιαῦται στόλαι καὶ τἄλλα. Dio Cassius, 63, 8, 4, mentions ἐμβάται in the list of Nero's outfit for his tour of artistic conquest through Greece, καὶ ὅπλα κιθάρας τε καὶ πλῆκτρα προσωπεῖά τε καὶ ἐμβάτας ἔφερον, and Nero wore them in the theatre, 63, 22, 4, εἶδον τὸν ἄνδρα ἐκεῖνον . . . ἐν τῷ τοῦ θεάτρου κύκλῳ καὶ ἐν τῇ ὀρχήστρᾳ ποτὲ μὲν κιθάραν ἔχοντα καὶ ὀρθοστάδιον καὶ κοθόρνους, ποτὲ δὲ ἐμβάτας καὶ προσωπεῖον. Liban. *Or.* 63, III, p. 385, 25 (Reiske), shows that they were high, τραγῳδοὺς δὲ ποίει βραχυτέρους γόνατα ἀποτέμνων, ἐπειδήπερ ἐμβάτας ἀναβάντες ἐμηχανήσαντο τοὺς ἄλλους ὑπεραίρειν. Suidas, s. v. Αἰσχύλος and φαιός, calls them τραγικοί, and also Ammon. *De diff. vocab.* s. v. ἐμβάδες and Thom. Mag. s. v. ἐμβάδες (see p. 150).

The frequency with which ἐμβάτης is used for the high-soled boot of tragedy unquestionably proves that Pollux is mistaken in calling it the shoe of comedy. The ἐμβάς is given a tragic connotation because of its similar spelling, and should be applied to the shoe of comedy which was nothing more than a common shoe.

κόθορνος. — It is very difficult to tell whether κόθορνος, which until the Imperial period was used only of a luxurious shoe for women, came to be applied in Roman times to the high-soled boot of tragedy, or whether it always retained its classical meaning and was wrongly called the tragic boot by authors under the influence of Roman customs. The latter view, to which I lean, is the view of Müller,[1] Arnold,[2] and Dierks.[3]

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[1] Müller, *Lehrbuch d. griech. Bühnenalterthümer*, p. 239.

[2] Arnold, in Baumeister's *Denkmäler*, p. 1853, n. 14.

[3] Dierks, *Trag. Histr. Hab.*, p. 12, note 3, p. 16.

The Romans, indeed, carried over from the Greek the idea of a boot for women or effeminate men.   Cf. Cic. *Fin.* 3, 14, 46 ;   *Phil.* 3, 6, 16. Verg. *Geor.* 2, 8.   Vell. 2, 82, 4.   Tac. *Ann.* 11, 31.   But they extended the meaning to 'hunting-boot' (cf. Verg. *Aen.* 1, 337 ;   *Ecl.* 7, 32) and to 'high-soled shoe of tragedy,' which is by far the commonest meaning of *cothurnus.*[1]   This was its meaning as early as the latter half of the first century B.C.   Hor. *C.* 2, 1, 9–12,

> Paulum severae musa tragoediae
> desit theatris ; mox ubi publicas
> res ordinaris, grande munus
> Cecropio repetes coturno, —

and by the time of Quintilian (*Inst. Or.* 6, 1, 36) the word was used in proverbs.   To Horace's mind *cothurnus* meant the buskin of Greek tragedy, as his words above quoted show.   How this change occurred it is difficult to discover.   My own theory is that when the high-soled shoe, the ἐμβάτης, was introduced into tragedy, the Romans called it by the name of a Greek shoe with which they were already familiar, which in general shape and decoration (for the κόθορνος was presumably the most richly ornamented of all Greek shoes) corresponded with the high-soled boots which the tragic actors then assumed.

Greek literature supports this theory, for it is the authors most under Roman influence who use κόθορνος of the high-soled tragic boot. Lucian, whose usual word is ἐμβάτης, uses the word in this sense twice, *Pro imag.* 3, εἴ τις ὑποδησάμενος κοθόρνους μικρὸς αὐτὸς ὢν ἐρίζοι περὶ μεγέθους τοῖς ἀπὸ ἰσοπέδου ὅλῳ πήχει ὑπερέχουσιν, and the doubt-ful passage *Somn.* 26, καὶ τῶν κοθόρνων (*v. l.* ἐμβάδων) τὴν ὑπόδεσιν ἀμορφοτάτην καὶ οὐ κατὰ λόγον τοῦ ποδός.   Pollux so uses the word twice, 4, 115, καὶ τὰ ὑποδήματα κόθορνοι μὲν τὰ τραγικὰ καὶ ἐμβάδες, ἐμβάται δὲ τὰ κωμικά· 7, 85, ἐμβάδες· εὐτελὲς μὲν τὸ ὑπόδημα,

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[1] Var. *Sat. Frg.* 10 of *Virgula Divina*, p. 237, 4 (Riese).   Prop. 2 (3), 34, 41. Hor. *C.* 2, 1, 12; *Sat.* 1, 5, 64; *A. P.* 80, 280.   Verg. *Ecl.* 8, 10.   Ovid, *Am.* 1, 15, 15; 2, 18, 15, 18; 3, 1, 14, 31, 45, 63; *Fast.* 5, 348; *Ib.* 531, 595; *Ex Pont.* 4, 16, 29; *Rem. Am.* 375;   *Trist.* 2, 393, 553, 554.   Sen. *Dial.* 9, 11, 8; *Ep.* 1, 8, 8; 9, 76, 31.   Plin. *N. H.* 35, 10 (36), 111.   Juv. 6, 506, 634; 7, 72; 15, 29. Quint. *Inst. Or.* 6, 1, 36; 10, 1, 68.   Mart. 3, 20, 7; 5, 5, 8; 5, 30, 1; 7, 63, 5; 8, 3, 13; 8, 18, 7; 11, 9, 1.   Tac. *De Or.* 10.   Suet. *Cal.* 52.   And others.

Θράκιον δὲ τὸ εὕρημα, τὴν δὲ ἰδέαν κοθόρνοις ταπεινοῖς ἔοικεν. And it is found in the anonymous *Vita Aeschyli* (Westermann, p. 121, 79), μείζοσί τε τοῖς κοθόρνοις (τοὺς ὑποκριτὰς) μετεωρίσας. On the contrary, it is used in its classical sense by Lucian, *Amor.* 50 ; *Pseudol.* 16 ; Pollux, 7, 91 ; Plutarch, *Nic.* 2 ; Themistius, *Or.* 67 D ; and Zenob. 3, 93, who repeat the story about Theramenes which Xenophon first narrated. Dio Cassius, 63, 22, 4, uses κόθορνος of the cithara-player's shoe[1] and contrasts it with the ἐμβάτης of an actor (see p. 151). Pausanias, 8, 31, 4, describes a statue of Dionysus wearing the κόθορνος, κόθορνοί τε καὶ τὰ ὑποδήματά ἐστιν αὐτῷ καὶ ἔχει τῇ χειρὶ ἔκπωμα, τῇ δὲ ἑτέρᾳ θύρσον. The scholiasts (Ar. *Av.* 994 ; *Eccl.* 346 ; *Ran.* 47, 541 ; *Nub.* 361) call the κόθορνος a boot fitting either foot, and so the nickname of Theramenes, while the scholiast on Ar. *Ran.* 47 adds that it was the shoe for women, τινὲς ὅτι ὁ κόθορνος εἰς ἀμφοτέρους τοὺς πόδας ἁρμόζει, οἱ δὲ ὅτι ἀνδράσι καὶ γυναιξὶν ἁρμόττει. ἔνθεν καὶ Θηραμένης κόθορνος ἐλέγετο, ὅτι τοῖς καιροῖς καθομιλεῖν δύναται. ὁ δὲ Ξενοφῶν ἐν Ἑλληνικοῖς ἀμφοτέροις τοῖς ποσὶν ἁρμόζειν αὐτόν φησιν. ἐκπλήττεται δὲ ὁ Ἡρακλῆς ὁρῶν τὴν ἄτοπον ταύτην σκευήν, καὶ ὅτι τὰ ἄμικτα ἔμιξεν. ὁ μὲν γὰρ κροκωτὸς καὶ ὁ κόθορνος γυναικεῖά ἐστιν, ἡ δὲ λεοντῆ καὶ τὸ ῥόπαλον ἀνδρῷα. The lexicographers, likewise, have no thought of a tragic boot in their definitions of the word : Photius, s. v. κόθορνος, ὑπόδημα ἀρχαῖον κοινὸν ἀνδρῶν καὶ γυναικῶν ταὐτὸν ἀμφοτέροις τοῖς ποσὶν ἐφαρμόττον. *Etym. Magn.* s. v. κόθορνος, γυναικεῖον ὑπόδημα τετράγωνον τὸ σχῆμα, ἁρμόζον ἀμφοτέροις τοῖς ποσί· Suidas, s. v. κόθορνος· ὑπόδημα ἀμφοτεροδέξιον. Ἀριστοφάνης (*Av.* 991) '' τίς ὁ κόθορνος τῆς ὁδοῦ ; '' οἷον τί ὑποδησάμενος πάρει ; Hesychius, s. v. κόθορνος· ὑπόδημα ἀμφοτέροις τοῖς ποσὶ πεποιημένον, τινὲς δὲ καὶ ἀνδράσι καὶ γυναιξὶν ἐφαρμόττειν φασὶ τὸ ὑπόδημα τὸν κόθορνον· cf. Joannes Cinnamus, p. 128 B, κοθόρνου δίκην ἀεὶ μεθαρμόζῃ ταῖς τύχαις.

If now lexicographers and scholiasts never mention tragedy in connection with the κόθορνος, if it is called by them a shoe for women, if Pausanias supports them by giving the κόθορνος to Dionysus, and Dio Cassius makes it the elaborate footgear of a cithara-player, what are we to think of the evidence of Lucian and Pollux and the anonymous

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[1] Cf. a painting on a Boeotian vase published by Dumont et Chaplain, *Céramiques d. l. Grèce Propre*, I, pl. 14, Paris, 1888.

author of the *Vita Aeschyli* who stand alone in this period for κόθορνος as the high-soled boot of tragedy? As I have previously stated, I believe that they use κόθορνος with its Latin significance. Lucian, a native of a Romanized province, a traveller and resident in other parts of the Roman empire, probably is guilty here of a Latinism. ἐμβάτης is his ordinary word, once κόθορνος is the reading in a corrupt text, only once does he without doubt use the word in its Roman sense. It seems clearly an incorrect use of the Greek word. Pollux is plainly culpable of the self-same error. When he wrote his definition of κόθορνος, the Latin *cothurnus* had been the Roman name of the buskin for over two hundred years. Pollux did not take his interpretation immediately from the Latin, but from one who had received a thorough Roman education, Juba of Mauretania, whose work περὶ τῆς θεατρικῆς ἱστορίας was also used by the author of the *Vita Aeschyli*.[1] It is only natural, then, that in this point of scenic antiquities he should show the influence of Roman tradition. It is for these reasons that I doubt the applicability of the Greek κόθορνος to the boot of tragedy. When the evidence for ἐμβάτης is so strong and for κόθορνος so weak, it seems unlikely that κόθορνος had any meaning of "high-soled tragic boot."

κρηπίς. — A single unknown authority declares that actors wore κρηπῖδες. *Vita Sophoclis* (Westermann), p. 128, 30, φησὶ δὲ καὶ Ἴστρος τὰς λευκὰς κρηπῖδας αὐτὸν ἐξευρηκέναι, ἃς ὑποδοῦνται οἵ τε ὑποκριταὶ καὶ οἱ χορευταί. The fact that the chorus wear them as well as the actors would prove without need of further evidence that these shoes were not high-soled, if a sentence in Bekker's *Anecdota Graeca* did not declare the opposite, p. 273, 17, κρηπιδουργός· ὁ τὰς κρηπῖδας ἐργαζόμενος σκυτοτόμος. κρηπὶς δὲ εἶδος ὑποδήματος ἀνδρικοῦ, ὑψηλὰ ἔχοντος τὰ καττύματα. This evidence, at best untrustworthy, is negatived by a large number of reliable authorities who use the word in quite different senses: Aristocles ap. Athen. 14, 621 B, σεμνότερος δὲ τῶν τοιούτων ἐστὶ ποιητῶν ὁ ἱλαρῳδὸς καλούμενος . . . καὶ τὸ μὲν παλαιὸν ὑποδήμασιν ἐχρῆτο, ὥς φησιν ὁ Ἀριστοκλῆς, νῦν δὲ κρηπῖσιν. Lucian, *Rhet. Praec.* 15, ἡ κρηπὶς Ἀττικὴ καὶ γυναικεία, τὸ πολυσχιδές.

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[1] Rohde, *De Pollucis Fontibus*, p. 82, Leipzig, 1870. Dierks, *Trag. Histr. Hab.*, p. 12, note 3.

Pollux, 7, 85, κρηπῖδες· τὸ μὲν φόρημα στρατιωτικόν. Dio Cass. 77, 7, τοῖς ὅπλοις οἷς ποτε ἐπ᾽ ἐκείνου ἐκέχρηντο ὁπλίσαι . . . κράνος ἀμοβόειον, θώραξ λινοῦς τρίμιτος, ἀσπὶς χαλκῆ, δόρυ μακρὸν, αἰχμὴ βραχεῖα, κρηπῖδες, ξίφος. Heliodorus, *Aethiop.* 3, 3, κρηπὶς μὲν αὐτοῖς ἱμάντι φοινικῷ διάπλοκος ὑπὲρ ἀστράγαλον ἐσφίγγετο. Hesychius, s. v. κρηπίς· ὑπόδημα. Without further discussion it must be obvious that κρηπίς is incorrectly applied to the high-soled tragic boot.[1]

ὀκρίβας. — It cannot be denied that this word is used of the high-soled tragic boot in the period we are discussing: Luc. *Nero*, 9, εἰσπέμπει Νέρων ἐπ᾽ ὀκριβάντων τοὺς ἑαυτοῦ ὑποκριτάς. Philostratus, *Vit. Apoll.* 5, 9, (speaks of Nero) ἐφεστῶτα δ᾽ ὀκρίβασιν οὕτως ὑψηλοῖς· *Vit. Apoll.* 6, 11 (of Aeschylus), ὀκρίβαντος[2] δὲ τοὺς ὑποκριτὰς ἀνεβίβασεν· *Vit. Soph.* 1, 9, ἐσθῆτί τ᾽ αὐτὴν κατασκευάσας καὶ ὀκρίβαντι ὑψηλῷ καὶ . . . οἷς ἐπὶ σκηνῆς τε καὶ ὑπὸ σκηνῆς χρὴ πράττειν. Themistius, *Or.* 316 D, Αἰσχύλος δὲ τρίτον ὑποκριτὴν καὶ ὀκρίβαντας, and Hesychius, s. v. ὀκρίβας makes the ἐμβάτης synonymous with it. *Etym. Magn.* s. v. ὀκρίβαντες· ἐφ᾽ ὧν ἄκρων ἑστᾶσιν οἱ ὑποκριταί. These authorities establish beyond question such a meaning of ὀκρίβας, but it is more than likely that this grew out of a misunderstanding of the passage in Plato's *Symposium*, 194 B. The one direct commentary upon this passage, moreover, upholds the classic usage : Timaeus, *Lexicon Vocum Platonicarum* (Ruhnken) s. v. ὀκρίβας· πῆγμα τὸ ἐν τῷ θεάτρῳ τιθέμενον, ἐφ᾽ οὗ ἵστανται οἱ τὰ δημόσια λέγοντες. θυμέλη γὰρ οὐδέπω ἦν. λέγει γοῦν τις· λόγιόν ἐστι πῆξις ἐστορεσμένη ξύλων, εἶτα ἑξῆς ὀκρίβας δὲ ὀνομάζεται.

Of the six words we have discussed, then, only ἐμβάτης and ὀκρίβας can properly apply to the high-soled boot of tragedy. Both are used of a tragic shoe of the Imperial period ; and Aeschylus is called the inventor of this style of boot. But this invention is assigned to classical times only by Latin writers and such late Greeks as Philostratus, Themistius, the author of the *Vita Aeschyli*, and Suidas. By all others

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[1] Cf. Daremberg et Saglio, *Dict. d. Antiq.*, under *Crepida*.

[2] The text is ungrammatical as it stands. The sense is clear, for Philostratus never uses the word except in referring to the boot of tragedy. An article at the least would be required here; but the genitive is not so used with ἀναβιβάζω. I would suggest ἐπ᾽ ὀκρίβαντας.

the wearing of the high-soled tragic boot is treated as a custom of the times without reference to the age of the custom. On such fragile evidence as these give is based the modern theory of the use of the high-soled boot. And the evidence of two of these ought to be entirely disregarded, — of Suidas, because he tries to find a classical equivalent for the ἐμβάτης of Roman times in the ἀρβύλη, the incorrectness of which has already been shown; of Themistius, because his notice about the third actor is incorrect. And the others apparently have assigned this invention to Aeschylus because he was the first great dramatist and an innovator along many lines.

There is a story told about Aeschines as an actor in the anonymous life of that orator (*Vit. Aeschin.* 3, 26, Westerm.) which hardly needs comment, so obvious is it that the situation does not require a high-soled boot for its explanation: Δημοχάρης δὲ ὁ ἀδελφιδοῦς Δημοσθέ-νους, εἰ ἄρα πιστευτέον αὐτῷ λέγοντι περὶ Αἰσχίνου, φησὶν Ἰσχάνδρου τοῦ τραγῳδοποιοῦ τριταγωνιστὴν γενέσθαι τὸν Αἰσχίνην, καὶ ὑποκρινό-μενον Οἰνόμαον διώκοντα Πέλοπα αἰσχρῶς πεσεῖν, καὶ ἀναστῆναι ὑπὸ Σαννίωνος τοῦ χοροδιδασκάλου. Clumsiness or a trifling accident account for an actor's falling better than the use of a high-soled boot.

Literature of this period, therefore, does not furnish any convincing evidence that the high-soled boot was used in the classical period. For this late period itself the use is unquestionably established; but the words for the boot are perverted from their classical meaning, and probably in all cases but the ἐμβάτης are inexact names for even the boot of this period.

EVIDENCE FROM ART OF THE IMPERIAL AND LATER PERIODS

Does the *art* of this period prove the use of the high-soled boot in classic Greek tragedy? The sole appears with varying height in many works of art of this time, but their evidence is good only for the period in which they were finished. Only one of them can by any possibility furnish evidence on the classic usage.

The earliest appearance of the high-soled boot is on a marble base discovered in Halicarnassus.[1] Among the Muses who form the sculptural

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[1] Trendelenberg, *Der Musenchor (36 Progr. z. Winckelmannsfest d. arch. Gesellsch. z. Berlin)*, Plate, Berlin, 1876.

decoration of this basis, the tragic Muse is plainly distinguishable by the mask which she holds in her right hand. On her feet are ornamented boots whose soles would be in actual life (i. e. if the whole relief were enlarged to life-size) a little less than three inches.[1] Therefore, since this basis is dated at about the end of the second century B.C., the use of the high sole as a symbol at that time points to a much earlier introduction of it. Yet a use during fifty years would not carry the date of its introduction back earlier than the beginning of the Roman period.

Other reliefs help us to discover the shape and height of this high sole, but, being of a later date, they cannot be used as evidence for the fifth and fourth centuries. Such are the relief of the Apotheosis of Homer,[2] by Archelaos of Priene, on which sandals appear in place of boots, and these sandals, reckoned proportionately as before, not two inches high ; the relief of the Meditating Muse on the so-called Capitoline sarcophagus, now in the Louvre (No. 307),[3] whose shoes have three-inch soles ; the Pourtalés relief of a resting tragic actor of markedly Roman features whose highly ornamented shoes have soles less than an inch and a half high ;[4] the relief of Valerian Paterculus on

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[1] My figures are based on the proportion the high sole bears to the length of the leg from the bottom of the foot to the centre of the kneecap. This length I have called twenty inches, slightly in excess of the standard measurements (Dr. D. A. Sargent), but sufficiently close for approximate reckoning. I chose this length in preference to the total height of the figure as being more easily ascertainable when masks were worn.

[2] Published by Visconti, *Museo Pio-Clementino*, I, p. 97, pl. B, Rome, 1782. Poorly published by Overbeck, *Geschichte der griechischen Plastik*, II⁴, p. 465, fig. 226, Leipzig, 1894. See also Clarac, *Musée de Sculpture*, pl. 516, No. 1051, Paris, 1832–1834; Friedrichs-Wolters, *Gipsabg.*, No. 1629.

[3] Published by Visconti, *Museo Pio-Clementino*, I, pl. B, No. 2; Clarac, *Mus. d. Sculpt.*, pls. 205 and 514, No. 1049; Wieseler, *Theatergeb. u. Denkm.*, pl. IX, 2. These publications disagree in the representation of the high soles. Some make them of equal height, others of differing heights. If the unequal soles are correct, there may be in this an attempt to represent the fact that the actors wore soles proportionate to their importance,— protagonist higher than deuteragonist, etc. The wall paintings show this most plainly.

[4] Published by Panofka, *Antiques du Cabinet du Comte de Pourtalés-Gorgier*, pl. XXXVIII, Paris, 1834, with restorations; and by Wieseler, *Theatergeb. u. Denkm.*, pl. IV, 10. Cf. Daremberg et Saglio, *Dict. d. Antiq. s. v. Cothurnus.*

which the high sole appears to be about three inches high ;[1] a relief on a bronze mirror-case in which most of the figures, even the chorus, have shoes with soles from four to five inches high ;[2] a gem on which Melpomene is represented standing on pedestals nine or ten inches high.[3]

Because of its frequent use in hand-books to represent the dress of a tragic actor, a small ivory statuette found near Rieti has become very well known.[4] It is a mistake, however, to use this as evidence for the dress of a tragic actor in the classical period. It is a work of the Christian era and, therefore, to be used only as evidence for that period.[5] If we are to regard the supports under each foot as soles, we get the excessive height of seven inches for the soles. But it is a question whether these are not rather pegs to hold the statuette in some sort of base. The drapery is not treated as if hanging above these, but as if touching the ground ; and, besides, a high sole is represented above beneath the drapery and visible in the partly broken foot.

A mosaic of the first century A.D., in the Museo Pio-Clementino, represents pairs of tragic actors in full dress and with shoes whose soles vary in height from five to ten inches.[6] But we cannot regard this mosaic as exact. Both actors wear shoes of equal height, and paintings from Pompeii and Herculaneum contradict this.

These paintings must of necessity date earlier than 79 A.D., and many were undoubtedly finished in the first century B.C. Some of these

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[1] Published by Winckelmann, *Monumenti Antichi Inediti*, I, pl. 189, Rome, 1767; and by Wieseler, *Theatergeb. u. Denkm.*, pl. XIII, 1.

[2] Published and discussed by Arnold, *Platte mit scenischen Vorstellungen im Collegio Romano* (*Festgruss der phil. Gesellsch. z. Würzburg an die XXVI. Versammlung deutsch. Philol. u. Schulm.*, pp. 142–157), Würzburg, 1868.

[3] Published by Wieseler, *Theatergeb. u. Denkm.*, pl. IX, 3. Cf. Meineke, *Com. Gr. Frag.* II, 1, p. 91.

[4] First published in colors in the *Monumenti Inediti*, XI, pl. 13, and discussed by Robert, *Annali d. Inst.*, 1880, p. 206 ff.

[5] Robert, *Hall. Winckelmannspr.* 22, p. 22, n. 16, and Pottier, in Daremberg et Saglio, *Dict. d. Ant.*, p. 1546. But Haigh, *Att. Th.*, p. 272, and Bethe, *Proleg. z. Gesch d. Th.*, p. 326, use this as evidence for the classic period.

[6] Published in colors by Wieseler, *Theatergeb. u. Denkm.*, pls. VII, VIII, 1–11. Single groups and figures are published elsewhere : Navarre, *Dionysos*, frontispiece; Daremb. et Sagl., *Dict. d. Ant.* s. v. *Cothurnus ;* etc.

paintings, in which we plainly see the representation of scenes from tragedy, depict the actors *without* high soles. One set of wall-paintings of this type is declared by Maass to be copied from originals of the late fourth or early third centuries, the time of Ptolemy Philadelphos, and so may be used as evidence for the early Hellenistic period.[1] Some of the scenes are from comedies and satyr-plays, but many represent scenes from tragedy as if the actual drama were being performed. In these the absence of the high soles is noteworthy. If they had ever been used, they ought to have appeared in these paintings which represent the actual performance of a tragedy.

Two other paintings, one from Pompeii, the other from Herculaneum, may have fourth century originals. The proportions of the figures in the former[2] show that no elevated sole is thought of on these figures, although the feet are hidden under the long chitons. The other painting[3] represents an actor resting after his victory, still in full tragic dress, but his shoes have only ordinary soles.

The evidence to be derived from these three paintings is conditioned by the chance that they are not copied from classic originals. Robert believes in this classic origin,[4] and so does Leo, who maintains, however, that "Das Fehlen der Kothurne erklärt sich aus malerischen Gründen,"[5] an easy answer to give to all pieces of evidence where the high sole is not found. But no proof that the high-soled boot was worn in classical times can be based on such a statement.

The high-soled boots appear in two paintings of this period. But authorities refer them to the immediate period. No one proposes classic originals for them. One of these represents two men raised on soles which for the more important rôle have a height of four inches,

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[1] Maass, *Affreschi Scenici di Pompei, Annali d. Inst.*, 1881, pp. 109–159. Published in *Mon. Ined.* XI, pls. 30–32.

[2] Published by Presuhn, *Pompeji, Die neuesten Ausgrabungen von 1878–1881*, Part IX, pl. 4, Leipzig, 1882; Robert, *Hall. Winckelmannspr.* 22, p. 38.

[3] Published in *Pitture Antiche d'Ercolano*, IV, pl. 41, p. 195, Naples, 1765; Wieseler, *Theatergeb. u. Denkm.*, pl. IV, 12; Daremb. et Sagl., *Dict. d. Ant.*, fig. 2656; etc.

[4] Robert, *Hall. Winckelmannspr.* 22, p. 25 ff.

[5] Leo, *Rheinisches Museum*, XXXVIII, p. 344. Cf. Daremb. et Sagl. s. v. *Cothurnus*.

for the lesser of an inch and a quarter.[1]   The other represents two women with soles of the same relative height as in the previous painting.[2]

A famous painting from Cyrene is taken by Wieseler as evidence for the use of the high-soled boot.   But it needs only a single glance at his own reproduction, in which the peculiar objects he calls soles are plainly detached from the feet of the figures, to convince one that he is wrong.[3]

Finally we must consider the most important painting of all.   The reliefs, mosaics, and paintings hitherto examined have not been able to prove the existence of the high-soled tragic boot before the close of the Hellenistic period.   A small, painted marble plaque, however, discovered in Herculaneum, has been thought by Professor C. Robert sufficiently trustworthy evidence to compel him to change his whole theory of the high-soled boot and force him to acknowledge its use in the fifth century.   That victorious choregi often erected votive offerings to commemorate their victories, and that copies of these were made by later painters, is a fact already established by Reisch.[4]   One of these Robert finds in the plaque-painting from Herculaneum.[5]

His argument (p. 15) is " Drei Figuren mit Masken und im Bühnen-costüm, zwei von ihnen in erregter Conversation, was kann das anders sein als die Scene eines Dramas, und wozu konnte ein solches Tafelge-mälde anders dienen, denn als Weihgeschenk eines attischen Choregen für einen dramatischen Sieg? . . . Da es als Kunstwerk nicht gerade hervorragend ist, dürfen wir von vorn herein vermuthen, dass sich ein

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[1] Published by Wieseler, *Theatergeb. u. Denkm.*, pl. IX, 1. Described by Helbig, *Wandgemälde d. vom Vesuv verschütteten Städte Campaniens*, No. 1467, Leipzig, 1868.

[2] Published by Gell, *Pompeiana*, N. S. II, pl. LXXV; Wieseler, *Theatergeb. u. Denkm.*, pl. VIII, 12.   The best publication of the high soles alone is Wieseler, *op. cit.*, pl. A, 23.   Publication in colors by Niccolini, *Case di Pompei*, I, p. 27 (*Casa di Castore e Polluce*, pl. III), Naples, 1854.   See Helbig, *Wandgem.*, No. 1465.

[3] Wieseler, *Theatergeb. u. Denkm.*, pl. XIII, 2, pp. 100, 102.

[4] Reisch, *Griechische Weihgeschenke*, in *Abhandlungen des archäologisch-epigraphi-schen Seminares der Universität Wien*, VIII, p. 1 ff.

[5] Robert, *Hallisches Winckelmannsprogr.* 22, p. 14 ff.   Published here and in *Pitt. d' Ercol.* I, 4, p. 19; Wieseler, *Theatergeb. u. Denkm.*, pl. XI, 5.   Described by Helbig, *Wandgem.*, No. 1464.

anderes, vielleicht litterarisches Interesse an das Bild knüpfte und dass dieses der Grund war, weshalb man es copirte." He thinks that the style and technique of the piece would date its original in the fifth century. The copy, perhaps, was made from literary interest more than because of its beauty, which, indeed, as he remarks, is rather hard to see. Its literary interest is as a picture of a tragic scene and he identifies this exactly, as the *Hippolytus* of Euripides, 704 ff., and the three persons represented on the plaque, as Phaedra, the nurse, and the chorus-leader. This settled, he can date the original votive-offering in 428 B.C., the year of the victory of the *Hippolytus*. The picture itself shows us two figures of the same height and one a head taller than they, all apparently wearing masks. This difference in height leads Robert to think that concealed under the long chiton are six-inch soles. The relatively short arms support this idea. Therefore, he says, the actors of heroic rôles wore boots with six-inch soles in the fifth century. For the fourth century he admits that the ordinary shoe replaced the high-soled shoe and in that fact he sees a reflection of the comic or realistic tendency of the plays of Euripides. To the third century he grants the high sole again, not reaching its greatest height, however, until the full Imperial period.

Such is the irregular history of the tragic boot, as Robert conceives it. The arguments from the fourth century are so strong that he is compelled to grant to it the ordinary shoe. He builds the entire history of the tragic boot in the fifth century upon the one marble plaque from Herculaneum.

The unsupported character of this evidence is in itself a weakness; but there are other weak points in the argument based upon it. For, while pointing out that Euripides was responsible for the adoption of the *low* sole, he, nevertheless, identifies the scene in one of Euripides' own plays. Thus, by his own interpretation this plaque shows the costume of the Euripidean drama which, as he thinks, was the main factor in introducing the low-soled shoe.

Again, his identification of the scene, while it seems to fit the *Hippolytus*, is at the best pure conjecture. When we have only a small fraction of the plays of the great dramatists, it is hazardous to identify so general a scene in one of those plays and then not only date a possible original votive-offering by it, but build up from it a scheme of

costuming for that period of the drama.   The scene itself is capable of many interpretations, and, if considered to represent a quarrel between a nurse and her mistress, might fit many of the tragedies not preserved to us.   In fact, the scene is so very vague in its application that some authorities have even considered it a scene from comedy.[1]   Certainly the costume is not the typical one of tragedy.

That paintings, especially those from Pompeii and Herculaneum, were not careful in representations of height is known from many sources.[2]   Sculpture, too, as seen in the Parthenon Frieze, took liberties with the heights of human beings.   Importance was often represented by size ; so here it is not necessary to suppose the use of high-soled shoes because one figure is taller than the others.

Furthermore, the Piraeus Relief dates within twenty-five years of the time when the high sole was used according to Robert.   Can we believe that the costuming of actors should have changed so radically in that space of time?

Lastly, it must be remembered that Robert is basing his evidence of the high sole for the whole period of dramatic excellence on shoes that are *not to be seen*, and only probable because one figure is taller than the others, and disproportionate in itself.   When it is thought that poor and careless workmanship might account for this lack of proportion, it does indeed seem dangerous to place much confidence in so ill-painted a plaque.   Who, when he thinks of the majestic simplicity of Aeschylus and the sweet beauty of Sophocles, does not feel a shrinking in his heart at thought of their being represented by such hideous, disproportionate, padded creatures as these of Robert's picture, and turn with relief and contentment to the beautiful, simple dress of the three actors on the Piraeus Relief !   Far more appropriately would Robert's picture represent the drama when it had reached the point of decline it has in our own day, when naturalness had given way to padded show and extravagance, than that period when, like our own Elizabethan times, imaginations were strong, tastes simple, and the drama strong enough in itself not to need artificial support.

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[1] Wieseler, *Theatergeb. u. Denkm.*, p. 86.

[2] See Wieseler, *op. cit.*, pl. XI, 2; Niccolini, *Case di Pompei*, I, p. 14, pl. VI (*Casa di Poeta Tragico*).

My argument against the high-soled tragic boot is entirely in line with all the latest investigations in scenic antiquities. These all tend to show that the great dramas were presented much more simply than we had been in the habit of thinking. Only a circle at first about which sat the spectators watching the dancing. At first, also, no scenery; then rock and tomb and altar; finally, the palace itself (in reality the dressing-rooms); arrangements all of the simplest kind. Our own Elizabethan dramas furnish a close parallel. Without even a permanent theatre, are we to suppose the actors appeared looking like the creatures of Robert's picture?[1] For my part, I can not. The plaque offers too many doubts, is dated back into the fifth century on too many hypotheses, to satisfy me that it is reliable. Moreover, consider what these suppositions have led to: a six-inch sole for the fifth century, none for the fourth, back again in the third. Such a development is most unnatural, but Robert is forced to propose it by the strong evidence from the fourth century against the high sole and his belief in the reliability of the marble plaque from Herculaneum as evidence for the fifth century. The Piraeus Relief, on the other hand, is at the latest early fourth century work, with a strong probability of dating in the fifth century. It is a genuine original. Robert's original is a matter of hypothesis, and the high soles are hypothetical in this hypothesis. Removed so far from fact they vanish into nothingness. Consequently, no evidence for the use of the high-soled boot in the fifth century can be drawn from this plaque.

The art of the Imperial period, therefore, while proving conclusively the use of the high-soled boot in that period, cannot prove it for earlier times, and some of the evidence confirms the evidence from classic art that the high sole was not used in those times.

CONCLUSION

The conclusions to be drawn from this investigation may be briefly stated. We have found that this boot is first represented in art at the close of the second century B.C. and then appears as a symbol of tragedy,

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[1] In coming through the door of the seven-foot high proscenium of Oropus an actor in the full dress of onkos and high soles would have had to bend his head considerably to get out at all.

that it is first mentioned in literature of the Imperial period, and that its use in this period is certain. We can not be so positive with regard to the Hellenistic period, but the literature that we have knows no name for such a boot. For the fifth and fourth centuries we have more than a plausible argument against the use of such a boot. The literature of the period has no name for any special tragic boot; the drama is full of lively action which the six-inch and higher sole we are asked to believe in almost precludes; the art of the period never pictures it either as a symbol of tragedy with the mask or as the footwear of actors, and the most positive piece of evidence on either side of the question, the Piraeus Relief, represents actors with natural, ordinary boots.

For these reasons we are led to believe that the late writers on scenic antiquities among the Greeks mistakenly supposed that the stage costume in their own day had remained the same for over five hundred years, and that the origin of the high sole was to be found in classic Greek drama. Quite naturally they ascribed the invention to Aeschylus. The evidence which we have does not seem to support their statements. And on the ground of this evidence I base my argument that the high sole was the invention of centuries after the classical period.[1]

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[1] How or when this came in, I am unable to say. The best discussion of this point is to be found in the *Hallisches Winckelmannsprogramm*, 22 (1898), p. 25 ff., by Professor Carl Robert.

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